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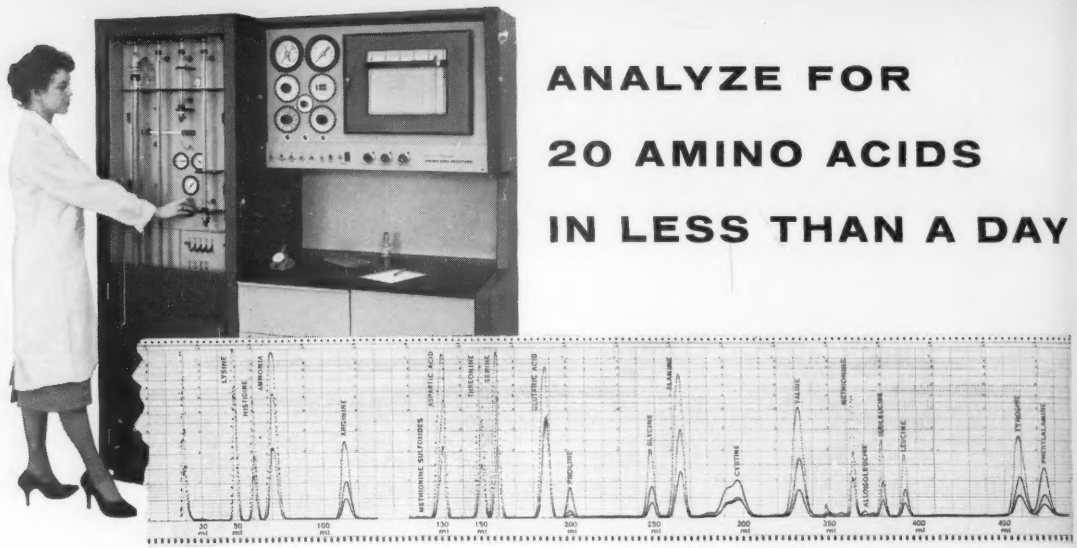

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SCIENCE

Cover

Thiocarbamide crystal. Dark-field illumination (× 575). A drop of solution is kept under observation until a suitable arrangement appears. The solvent determines the pattern produced during crystallization. Rapid crystallization produces a coarse pattern, slow crystallization a fine one. [L. C. Massopust, Marquette University School of Medicine]

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Letters

Chronology of the Last Glaciation

James B. Griffin in his extremely interesting article, "Some prehistoric connections between Siberia and America" [*Science* **131**, 801 (1960)], states that "the Wisconsin ice advance is thought by some Pleistocene students to have begun about 50,000 B.C., followed by a warmer period corresponding to the Würm interstadial in Europe. This may have provided an ice-free corridor east of the Rockies some 30,000 years ago."

I have no doubt that Griffin is right about the thinking of some, if not many, Pleistocene students. Apparently a short but important paper by H. Tauber and H. de Vries [*Eiszeitalter und Gegenwart* **9**, 69 (1958)] has received less attention than it deserved. According to these authors, samples for radiocarbon dating from the Würm interstadial deposit at Brörup, Jutland, showed no significant activity after thorough decontamination. "This means," they write, "that the interstadial at Brörup and the preceding cold period are older than 50,000 B.C." And, one may add, perhaps much older.

It has been [D. B. Ericson and G. Wollin, *Micropaleontol.* **2**, 257 (1956)] and still is my guess that the Würm I-II or Brörup interstadial is represented in the deep-sea sediments of the North Atlantic by a well-defined faunal zone containing low-latitude species of planktonic foraminifera among which *Globorotalia menardii flexuosa* is especially abundant. From the stratigraphical position of this zone, at the base of a relatively thick layer of sediment with cold-water species throughout, which lies, in turn, directly beneath postglacial sediment, it is difficult to see how it could be anything else than the Würm interstadial. Now, extrapolation of rates of sediment accumulation determined by radiocarbon dating of many samples from long sediment cores from various parts of the Atlantic, Caribbean, and Gulf of Mexico has shown that the time interval represented by the zone containing *G. menardii flexuosa*, or the Flexuosa zone, came to an end about 65,000 years ago.

Of course, my correlation of the Flexuosa zone with the Würm interstadial may be wrong in spite of its apparent plausibility. Even so, the important fact remains that Tauber and de Vries have shown conclusively that the climatic amelioration which separated the early and late Wisconsin glaciations occurred more than 50,000 years ago, and that therefore the short chronology of the last glaciation must be abandoned. Accordingly we conclude that if early man entered America during the Würm interstadial, he must have done so at least 50,000 years ago, and perhaps no less than 65,000 years ago. This does not impair Griffin's argument regarding the time of man's appearance in America. If anything, this longer chronology strengthens his conclusion that man did not enter America during the interstadial between the early and late Wisconsin glaciations.

DAVID B. ERICSON
Lamont Geological Observatory, Palisades, New York

Emotionality and Fear

Harlow and Zimmermann's description of "Affectional responses in the infant monkeys" [*Science* **130**, 421 (1959)] was a gem, but it did, I believe, contain a minor flaw.

Although it clearly described "affectional responses," which are indeed emotional responses, when the term *emotionality* was used, it seemed that its meaning was limited to the disruptive emotion of fear. This can be seen in the use of the term *emotionality index* rather than *fear index*, and in the following statement (p. 425): "Children in the first group (mother present) were much less *emotional* [italics mine] and participated much more fully in the play activity than those in the second group (mother absent)." This first group was not less "emotional" but less fearful; presumably, if the first group was happier, it could also be called *more* emotional than the second.

Harlow and Zimmermann seem implicitly and inaccurately to equate emotionality with fear, an equation which would lead us to see courage, for example, as equivalent to emotionlessness. A very fine critique of the theory "according to which emotions are disorganized or disruptive states" is to be found in V. J. McGill's *Emotions and Reason* (Thomas, Springfield, Ill., 1954).

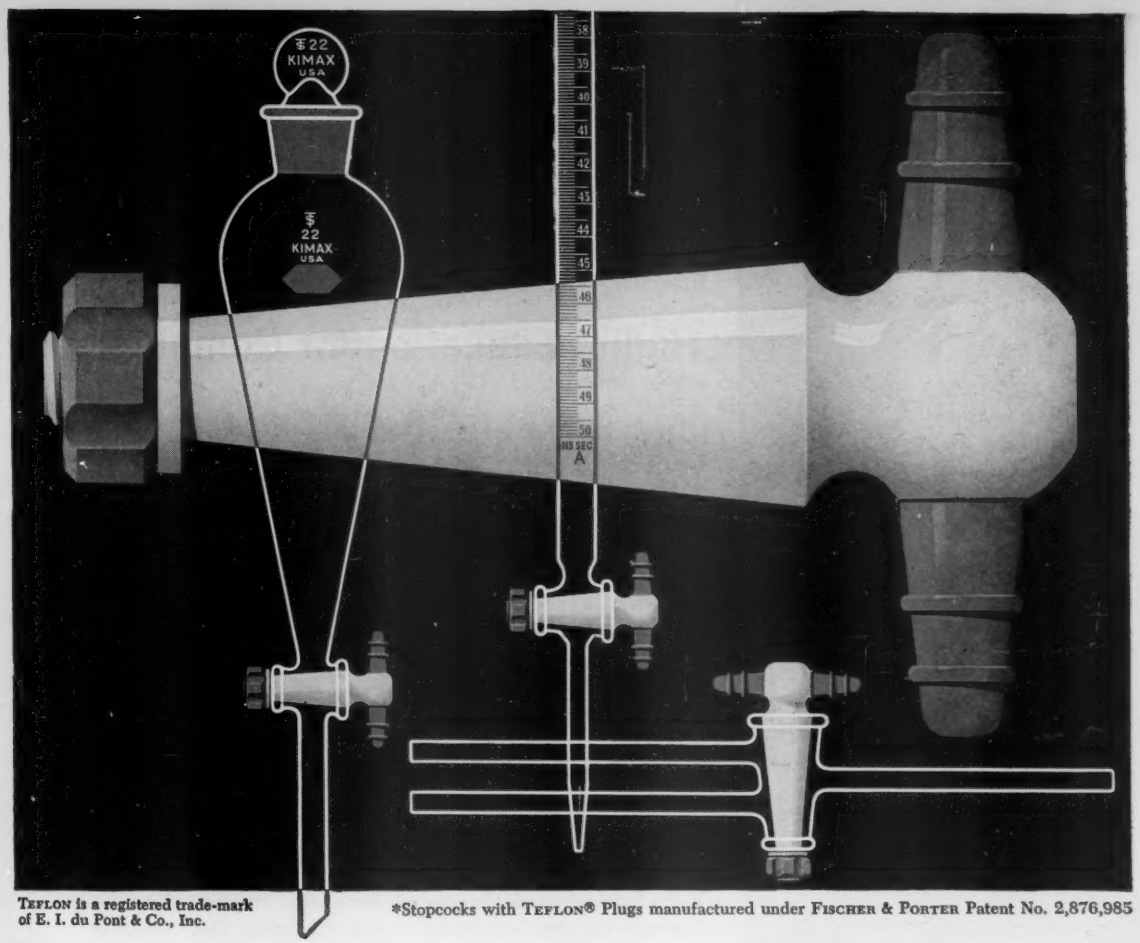
Hence I think their fine article would be even finer if this inaccuracy in the use of the concept "emotionality" were clarified.

NATHANIEL S. LEHRMAN
15 Canterbury Road, Great Neck, New York

I am in agreement with Lehrman's position and have long been an opponent of those psychologists who would identify and define emotion as "disorganized and disruptive states." In an earlier paper [*Am. Psychologist* **12**, 673 (1958)], I unequivocally define love as an emotion, and I still subscribe to this theoretical position.

The term *fear index* would have been

(Continued on page 1740)



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Meanwhile, Down in the Valley

Last month saw the collapse of negotiations at the summit, with accusations from Moscow that Washington had wrecked the Paris meeting by staging the 1 May flight of the U-2 reconnaissance plane, and accusations from Washington that Moscow had been determined all along that nothing should come of the Big Four conference. At a lesser height, the quarrel moved into the United Nations Security Council where the Soviet Union sought unsuccessfully to get the United States indicted for "aggression." Down in the valley, however, at the level of physics, medical science, and the arts, the world was treated to the sight of another aspect of the East-West dialogue continuing undisturbed.

Premier Khrushchev may have withdrawn his invitation to President Eisenhower to visit him in the Kremlin, but exchange programs that had started before the crisis proceeded right through it. American physicists visited various high-energy research centers in the Soviet Union, American medical scientists attended a conference in Moscow on poliomyelitis, and the American violinist Isaac Stern played encores to Russian applause. In this country, Soviet scientists specializing in thermonuclear research visited the James Forrestal Laboratory at Princeton University and other laboratories, and Soviet medical scientists attended conferences at Johns Hopkins and elsewhere on heart disease and related problems.

The hope that exchange programs will promote mutual trust is equally strong when relations on the political level are not so favorable as when they are favorable. An increase in mutual trust at the top can lead to greater opportunities for exchange below, and bringing people together at lower levels in activities that command common loyalties can affect relations above. Exchange may promote mutual confidence by correcting the distorted images that East and West may have of each other. We do not expect that at the mere sight of our magical presence all Russia will break out into "The Stars and Stripes Forever," but we can hope that those with eyes to see will learn that we are not a nation of hungry, exploited workers ripe for revolt against our secretary-chasing, cigar-chomping masters. And, for our part, in observing the Soviet way of life, we will never tire of watching demonstrations that the Russia of Khrushchev is not that of Stalin.

An increase in mutual trust generated by exchange programs can also bear on more specific American goals such as carrying out a step-by-step disarmament program. Part of the theory underlying this program is that the confidence between East and West developed in one step will furnish the confidence needed for the next step. But since new negotiations can also be new sources of suspicion, independent methods for building mutual trust are also necessary.

President Eisenhower has called for a continuation of cultural contacts and "businesslike dealings" with Soviet leaders, while Premier Khrushchev has found this statement of "positive value." This is good news. In the technical portion of exchange, our hope is that scientists, in seeking broader avenues of communication, will help reduce the tensions that their own achievements have made so dangerous.—J.T.

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The Effect of Dream Deprivation

The need for a certain amount of dreaming each night is suggested by recent experiments.

William Dement

About a year ago, a research program was initiated at the Mount Sinai Hospital which aimed at assessing the basic function and significance of dreaming. The experiments have been arduous and time-consuming and are still in progress. However, the results of the first series have been quite uniform, and because of the length of the program, it has been decided to issue this preliminary report.

In recent years, a body of evidence has accumulated which demonstrates that dreaming occurs in association with periods of rapid, binocularly synchronous eye movements (*1–3*). Furthermore, the amount and directional patterning of these eye movements and the associated dream *content* are related in such a way as to strongly suggest that the eye movements represent scanning movements made by the dreamer as he watches the events of the dream (*3*). In a study of undisturbed sleep (*4*), the eye-movement periods were observed to occur regularly throughout the night in association with the lightest phases of a cyclic variation in depth of sleep, as measured by the electroencephalograph. The length of individual cycles averaged about 90 minutes, and the mean duration of single periods of eye movement was about 20 minutes. Thus, a typical night's sleep includes four or five periods of dreaming, which account for about 20 percent of the total sleep time.

One of the most striking facts apparent in all the works cited above was that a very much greater amount of dreaming occurs normally than had heretofore been realized—greater both from the standpoint of frequency and duration in a single night of sleep and in the invariability of its occurrence from night to night. In other words, dreaming appears to be an intrinsic part of normal sleep and, as such, although the dreams are not usually recalled, occurs every night in every sleeping person.

A consideration of this aspect of dreaming leads more or less inevitably to the formulation of certain rather fundamental questions. Since there appear to be no exceptions to the nightly occurrence of a substantial amount of dreaming in every sleeping person, it might be asked whether or not this amount of dreaming is in some way a necessary and vital part of our existence. Would it be possible for human beings to continue functioning normally if their dream life were completely or partially suppressed? Should dreaming be considered necessary in a psychological sense or a physiological sense or both?

The obvious attack on these problems was to study subjects who had somehow been deprived of the opportunity to dream. After a few unsuccessful preliminary trials with depressant drugs, it was decided to use the somewhat drastic method of awakening sleeping subjects immediately after the onset of dreaming and to continue this procedure throughout the night, so that each dream period would be artificially terminated right at its beginning.

Subjects and Method

The data in this article are from the first eight subjects in the research program, all males, ranging in age from 23 to 32. Eye movements and accompanying low-voltage, nonspindling electroencephalographic patterns (*4*) were used as the objective criteria of dreaming. The technique by which these variables are recorded, and their precise relationship to dreaming, have been extensively discussed elsewhere (*2, 4*). Briefly, the subjects came to the laboratory at about their usual bedtime. Small silver-disk electrodes were carefully attached near their eyes and on their scalps; then the subjects went to sleep in a quiet, dark room in the laboratory. Lead wires ran from the electrodes to apparatus in an adjacent room upon which the electrical potentials of eye movements and brain waves were recorded continuously throughout the night.

Eye movements and brain waves of each subject were recorded throughout a series of undisturbed nights of sleep, to evaluate his base-line total nightly dream time and over-all sleep pattern. After this, recordings were made throughout a number of nights in which the subject was awakened by the experimenter every time the eye-movement and electroencephalographic recordings indicated that he had begun to dream. These "dream-deprivation" nights were always consecutive. Furthermore, the subjects were requested not to sleep at any other time. Obviously, if subjects were allowed to nap, or to sleep at home on any night in the dream-

The author is on the staff of Mount Sinai Hospital, New York, in the department of psychiatry.

deprivation period, an unknown amount of dreaming would take place, offsetting the effects of the deprivation. On the first night immediately after the period of dream deprivation, and for several consecutive nights thereafter, the subject was allowed to sleep without disturbance. These nights were designated "recovery nights." The subject then had a varying number of nights off, after which he returned for another series of interrupted nights which exactly duplicated the dream-deprivation series in number of nights and number of awakenings per night. The only difference was that the subject was awakened in the intervals between eye-movement (dream) periods. Whenever a dream period began, the subject was allowed to sleep on without interruption, and was awakened only after the dream had ended spontaneously. Next, the subject had a number of recovery nights of undisturbed sleep equal to the number of recovery nights in his original dream-deprivation series. Altogether, as many as 20 to 30 all-night recordings were made for each subject, most of them on consecutive nights. Since, for the most part, tests could be made on only one subject at a time, and since a minute-by-minute all-night vigil was required of the experimenter to catch each dream episode immediately at its onset, it can be understood why the experiments have been called arduous and time-consuming.

Table 1 summarizes most of the pertinent data. As can be seen, the total number of base-line nights for the eight subjects was 40. The mean sleep time for the 40 nights was 7 hours and 2 minutes, the mean total nightly dream time was 82 minutes, and the mean percentage of dream time (total dream time to total sleep time $\times$ 100) was 19.4. Since total sleep time was not held absolutely constant, percentage figures were routinely calculated as a check on the possibility that differences in total nightly dream time were due to differences in total sleep time. Actually, this is not a plausible explanation for any but quite small differences in dream time, because the range of values for total sleep time for each subject turned out to be very narrow throughout the entire study. When averaged in terms of individuals rather than nights, the means were: total sleep time, 6 hours 50 minutes; total dream time, 80 minutes; percentage of dream time, 19.5; this indicates that the figures were not skewed by the disparate number of base-line nights per subject. The remarkable uniformity of the findings for individual nights is demonstrated by the fact that the standard deviation of the total nightly dream time was only plus or minus 7 minutes.

Progressive Increase in Dream "Attempts"

The number of consecutive nights of dream deprivation arbitrarily selected as a condition of the study was five. However, one subject left the study in a flurry of obviously contrived excuses after only three nights, and two subjects insisted on stopping after four nights but consented to continue with the recovery nights and the remainder of the schedule. One subject was pushed to seven nights. During each awakening the subjects were required to sit up in bed and remain fully awake for several minutes. On the first nights of dream deprivation, the return to sleep generally initiated a new sleep cycle, and the next dream period was postponed for the expected amount of time. However, on subsequent nights the number of forced awakenings required to suppress dreaming steadily mounted. Or, to put it another way, there was a progressive increase in the number of attempts to dream. The number of awakenings required on the first and last nights of deprivation are listed in Table 1. *All* the subjects showed this progressive increase, although there was considerable variation in the starting number and the amount of the increase. An important point is that each awakening was preceded by a minute or two of dreaming. This represented the time required for the experimenter to judge the emerging record and make the decision to awaken the subject after he first noticed the beginning of eye movements. In some cases the time was a little longer, as when an eye-movement period started while the experimenter was looking away from the recording apparatus. It is apparent from this that the method employed did not constitute absolute dream deprivation but, rather, about a 65- to 75-percent deprivation, as it turned out.

Nightly Dream Time Elevated after Deprivation

The data on the first night of the dream deprivation recovery period are summarized for each subject in Table 1. As was mentioned, one subject had quit the study. The mean total dream time on the first recovery night was 112 minutes, or 26.6 percent of the total mean sleep time. If the results for two subjects who did not show marked increases on the first recovery night are excluded, the mean dream time is 127 minutes or 29 percent, which represents a 50-percent increase over the group base-line mean. For all seven subjects together, on the first recovery night the increase in percentage of dream time over the base-line mean (Table 1, col. 3, mean percentage figures; col. 10, first recovery night percentages) was significant at the $p < .05$ level in a one-tail Wilcoxin matched-pairs signed-ranks test (*5*).

It is important to mention, however, that one (S.M. in Table 1) of the two subjects alluded to above as exceptions was not really an exception because, although he had only 1 hour 1 minute of dreaming on his first recovery night, he showed a marked increase on *four* subsequent nights. His failure to show a rise on the first recovery night was in all likelihood due to the fact that he had imbibed several cocktails at a party before coming to the laboratory so that the expected increase in dream time was offset by the depressing effect of the alcohol. The other one of the two subjects (N.W. in Table 1) failed to show a significant increase in dream time on any of five consecutive recovery nights and therefore must be considered the single exception to the over-all results. Even so, it is hard to reconcile his lack of increase in dream time on recovery nights with the fact that during the actual period of dream deprivation he showed the largest build-up in number of awakenings required to suppress dreaming (11 to 30) of any subject in this group. One may only suggest that, although he was strongly affected by the dream loss, he could not increase his dream time on recovery nights because of an unusually stable basic sleep cycle that resisted modification.

The number of consecutive recovery nights for each subject in this series of tests was too small in some cases, mainly because it was naively supposed at the beginning of the study that an increase in dream time, if it occurred, would last only one or two nights. One subject had only one recovery night, another two, and another three. The dream time was markedly elevated above the base-line on all these nights. For how many additional nights each of these three subjects would have maintained an elevation in dream time

Table 1. Summary of experimental results. *TST*, total sleep time; *TDT*, total dream time.

Mean and range, base-line nights			Dream-deprivation nights (No.)	Awakenings (No.)		Dream-deprivation recovery nights				First control recovery night		
							First night					
TST	TDT	Percent		First night	Last night	No.	TST	TDT	Percent	TST	TDT	Percent
				Subject W. T. (4 base-line nights)								
6h36m	1h17m	19.5	5	8	14	1	6h43m	2h17m	34.0	6h50m	1h04m	15.6
6h24m–6h48m	1h10m–1h21m	17.0–21.3										
				Subject H. S. (5 base-line nights)								
7h27m	1h24m	18.8	7	7	24	2	8h02m	2h45m	34.2	8h00m	1h49m	22.7
7h07m–7h58m	1h07m–1h38m	15.4–21.8										
				Subject N. W. (7 base-line nights)								
6h39m	1h18m	19.5	5	11	30	5	6h46m	1h12m	17.8	7h10m	1h28m	20.2
5h50m–7h10m	1h11m–1h27m	17.4–22.4										
				Subject B. M. (6 base-line nights)								
6h59m	1h18m	18.6	5	7	23	5	7h25m	1h58m	26.3	7h48m	1h28m	18.8
6h28m–7h38m	0h58m–1h35m	14.8–22.2										
				Subject R. G. (10 base-line nights)								
7h26m	1h26m	19.3	5	10	20	5	7h14m	2h08m	29.5	7h18m	1h55m	26.3
7h00m–7h57m	1h13m–1h46m	16.9–22.7										
				Subject W. D. (4 base-line nights)								
6h29m	1h21m	20.8	4	13	20	3	8h53m	2h35m	29.0			
5h38m–7h22m	1h08m–1h32m	17.8–23.4										
				Subject S. M. (2 base-line nights)								
6h41m	1h12m	17.9	4	22	30	6	5h08m	1h01m	19.8	6h40m	1h07m	16.8
6h18m–7h04m	1h01m–1h23m	16.2–19.3					6h32m*	1h50m*	28.1*			
				Subject W. G. (2 base-line nights)								
6h16m	1h22m	20.8	3	9	13							
6h08m–6h24m	1h17m–1h27m	20.7–20.9										

*Second recovery night (see text).

can only be surmised in the absence of objective data. All of the remaining four subjects had five consecutive recovery nights. One was the single subject who showed no increase, two were nearing the base-line dream time by the fifth night, and one still showed marked elevation in dream time. From this admittedly incomplete sample it appears that about five nights of increased dreaming usually follow four or five nights of dream suppression achieved by the method of this study.

Effect Not Due to Awakening

Six of the subjects underwent the series of control awakenings—that is, awakenings during non-dream periods. This series exactly duplicated the dream-deprivation series for each subject in number of nights, total number of awakenings, and total number of awakenings per successive night. The dream time on these nights was slightly below base-line levels as a rule. The purpose of this series was, of course, to see if the findings following dream deprivation were solely an effect of the multiple awakenings. Data for the first recovery nights after nights of control awakenings are included in Table 1. There was no significant increase for the group. The mean dream time was 88 minutes, and the mean percentage was 20.1. Subsequent recovery nights in this series also failed to show the marked rise in dream time that was observed after nights of dream deprivation. A moderate increase found on four out of a total of 24 recovery nights for the individuals in the control-awakening group was felt to be a response to the slight reduction in dream time on control-awakening nights.

Behavioral Changes

Psychological disturbances such as anxiety, irritability, and difficulty in concentrating developed during the period of dream deprivation, but these were not catastrophic. One subject, as was mentioned above, quit the study in an apparent panic, and two subjects insisted on stopping one night short of the goal of five nights of dream deprivation, presumably because the stress was too great. At least one subject exhibited serious anxiety and agitation. Five subjects developed a marked increase in appetite during the period of dream deprivation; this observation was supported by daily weight measurements which showed a gain in weight of 3 to 5 pounds in three of the subjects. The psychological changes disappeared as soon as the subjects were allowed to dream. The most important fact was that *none* of the observed changes were seen during the period of control awakenings.

The results have been tentatively interpreted as indicating that a certain amount of dreaming each night is a necessity. It is as though a pressure to dream builds up with the accruing dream deficit during successive dream-deprivation nights—a pressure which is first evident in the increasing frequency of attempts to dream and then, during the recovery period, in the marked increase in total dream time and percentage of dream time. The fact that this increase may be maintained over four or more successive recovery nights suggests that there is a more or less quantitative compensation for the deficit. It is possible that if the dream suppression were carried on long enough, a serious disruption of the personality would result (*6*).

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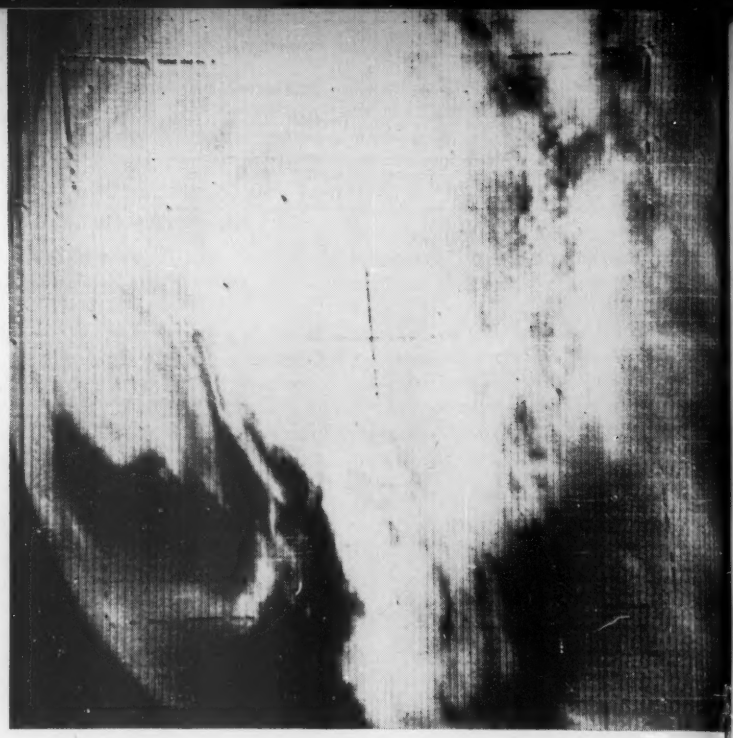
Tiros Reveals Cloud Formations

Tiros I, the experimental weather satellite, in the first month of its operation, orbited the earth about 450 times and took about 9000 pictures of various cloud formations (*1*). The observing equipment has been described elsewhere (*2*). The pictures have revealed a large degree of organization in cloud systems over much of the earth's surface. The most striking patterns are spiral cloud bands associated with large storms, some about 1000 miles in diameter, observed over the United States, the North Atlantic Ocean, the North and South Pacific Oceans, and the Indian Ocean (*3*). Seven of these cloud vortices are shown in the accompanying figures.



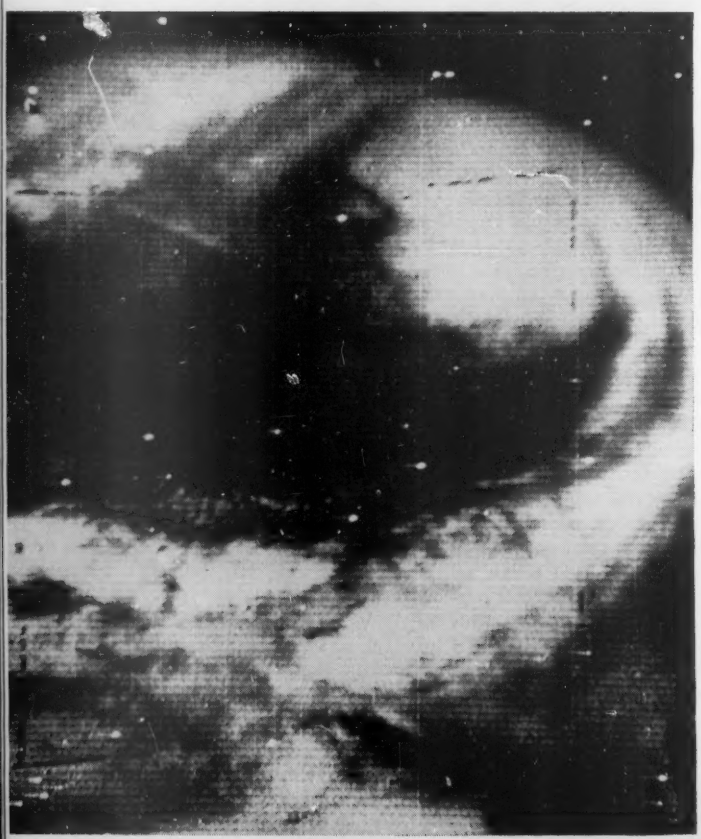
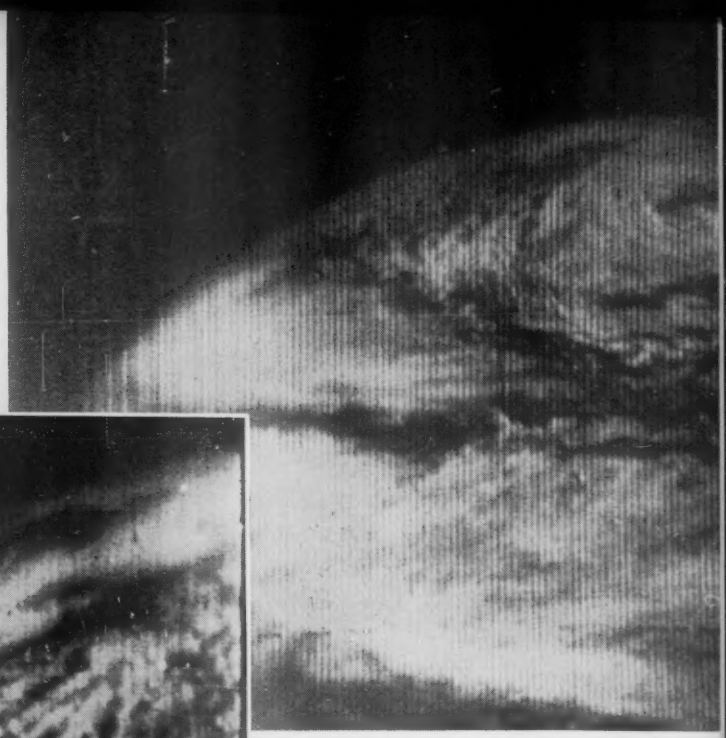
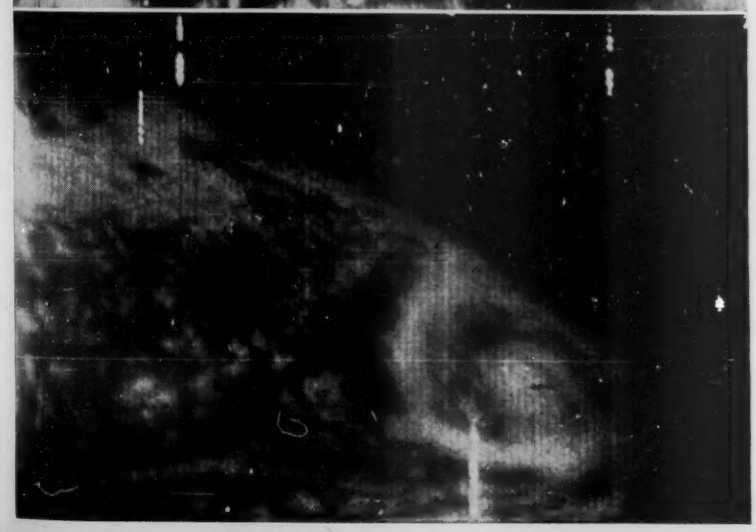


Figure 1 (above) shows the cloud formation associated with one of the first storms observed by Tiros on the launching date, 1 April 1960. The storm was located 120 miles east of Cape Cod (Massachusetts) and had a well-marked counterclockwise rotation. Dry, cloudless continental air, indicated by the dark area in the lower left, is streaming eastward off the middle Atlantic coast, while moist air from the south moves northward and westward around the storm center. These air movements produced widespread clouds and precipitation as far north as the Gulf of St. Lawrence.

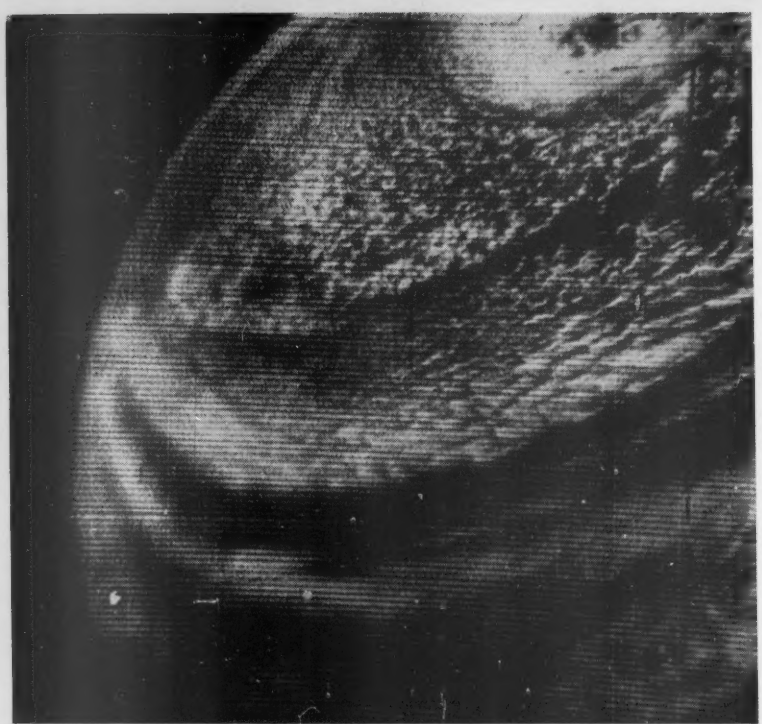
Figure 2 (left), which was taken on the same day, shows clouds associated with a large storm centered over southeastern Nebraska. Again, the central dark portion of the picture is an area where the ground is visible through the dry, cloudless air from Canada that is circulating counterclockwise about the storm center, while, to the east, the moist, warm air from the Gulf of Mexico enters the storm circulation, causing widespread rains and snows. A rather bright, broad band of clouds, located over the lower Mississippi Valley, is shown in the lower portion of the picture. Some of these clouds are quite bright, suggesting that they are of greater thickness. Thunderstorms were imbedded in this cloud mass.

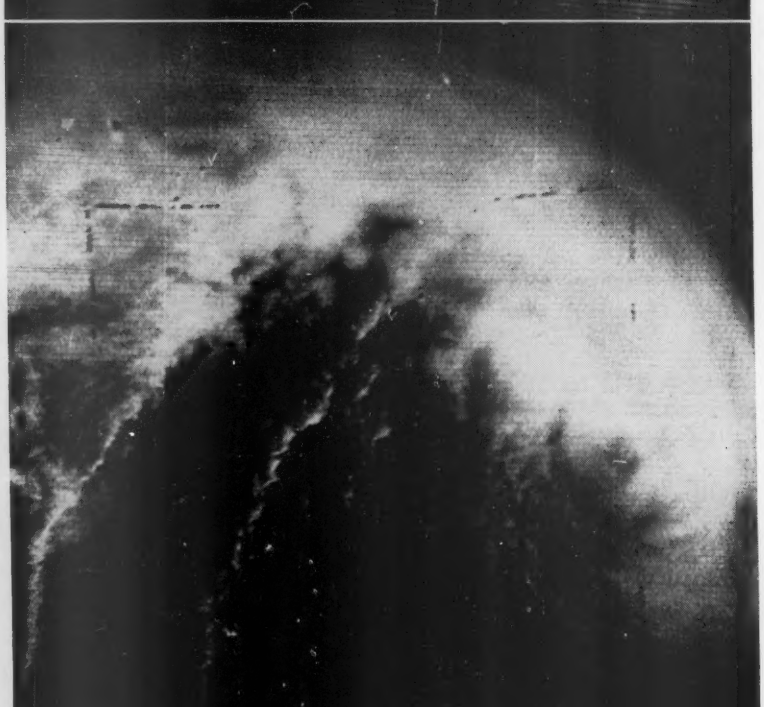
The third vortex, also observed on 1 April (Fig. 3, right), was iocated over the Gulf of Alaska, about 500 miles southeast of Kodiak Island. The vortex circulation is clearly indicated by the spiral array of cloud bands and a clear area in the center of the storm. In this oceanic storm, much more fine structure is visible than in the continental and coastal storms shown in Figs. 1 and 2.





Figures 4 and 5 (left, top and bottom) are views of the cloud cover associated with a large storm located about 400 miles west of Ireland on 2 and 3 April. This old, occluded storm had no fronts associated with it, but a marked banded structure is visible on 2 April (Fig. 4, top). The centrally located clouds in Fig. 4 are surrounded by a solid mass of stratiform clouds with a sharp boundary as revealed on adjacent frames taken during the same orbit of Tiros. Parts of this sharp, circular cloud boundary are seen in the arrow-like cloud near the top and in the lower right of Fig. 4. This cloud boundary is near and parallel to a region of a 60-knot wind speed maximum, on the 500-millibar constant pressure map (about 17,500 feet), which circled the storm. On the next day (Fig. 5, bottom) there seems to be one very large band winding around the center. Future photogrammetric measurements may reveal the nature of the change in cloud pattern from 2 to 3 April.





In Fig. 6 (top left) is shown another vortex, quite different from the others. This is a cloud vortex, about 1000 miles in diameter, located 800 miles west of southern California on 4 April. Here are superimposed bands of several different scales. Several wide bands separated by narrow clear areas may be clearly seen. Each of these individual large bands is composed of a series of smaller bands. The differences in band width illustrate the great variety in scale of motion which characterizes the atmosphere.

In Fig. 7 (bottom left) there is another type of vortex, this time observed in the Southern Hemisphere, where cyclonic storms rotate clockwise instead of counterclockwise. This storm, a typhoon which was known to meteorologists in that area for several days before Tiros was programmed to observe it, displays a pattern of spiral bandedness similar to precipitation echoes observed by radar. This storm was located about 300 miles north of the northern tip of New Zealand. The cloudless eye of the storm is quite visible as a dark spot on the right center of the picture. The storm was at the time moving southeastward, which is toward the upper right of the picture.

As Tiros observes future cloud-storm systems, it will be interesting to study the distinction between the fine-structure cloud-bandedness already observed in several maritime storms and the lack of such fine detail that is associated with the two continental or coastal storms mentioned above.

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Fallout in New York City during 1958

The data indicate that short-lived fission products make a major contribution to radiation dose rates.

George A. Welford and William R. Collins, Jr.

Since 1951 increasing emphasis has been placed on the off-site sampling and measurement of radioactive debris from nuclear detonations. The primary objectives have been the estimation of world-wide gamma-radiation dose and documentation of the deposition of strontium-90 as a major biological hazard. In practice, these two consequences of nuclear testing were calculated from daily total beta-activity levels. More recently, world-wide monthly collections have been made for direct radio-chemical measurements. Of the sampling sites in operation, New York City is of particular interest because of its dense population and its location in the latitude of relatively high deposition of debris.

This article describes the deposition of strontium-90, strontium-89, cesium-137, zirconium-95, cerium-144, yttrium-91, and tungsten-185 measured in New York City during 1958. The chemical state of the debris at the time of deposition is approximated through solubility studies. The effect of rainfall on fallout patterns is discussed, and the age and origin of each monthly fallout collection are estimated through nuclide ratios.

Methods

In January 1958, an auxiliary collection system was established at the Health and Safety Laboratory of the U.S. Atomic Energy Commission for the detailed analysis of fallout debris in New York City. Three collection techniques were used to minimize inter-collector variances, and replicate collections were made to increase reliability. The sampling systems employed were high-walled stainless steel pots and two funnel systems (*1*). The funnel collectors exposed a cylindrical funnel with a conical base to the atmosphere. In one system the material collected was washed directly into a reservoir by rain or by added wash water, while the debris in the other system (an ion-exchange funnel collector) was washed through a paper-pulp filter and a mixed-bed ion exchanger to extract the nuclear debris.

A sequential radiochemical scheme was developed in order to analyze the total sample (3 to 5 grams of ash per month in New York) for the isotopes listed above (*2*). Finally, data collection and calculations were performed by means of an automatic data processing system built around two general digital computer programs. Experimental data were tabulated as the analyses proceeded and were punched into I.B.M. cards to be fed into the computer. The general calculations are fitted to specific analyses by substituting constants pertinent to individual nuclides (*3*).

Fallout Deposition in New York City

Until recently, fallout measurements at sites removed from testing areas were primarily concerned with concentrations of strontium-90 and cesium-137 and were expressed either in terms of cumulative levels or deposition rates. Since many complex biological factors must be considered as strontium and cesium pass through the food chain, it is agreed that deposition of these fission products is not an adequate measure of their hazard to man. When the route is from soil to plants to cattle to milk, entry into the body is a function of the accumulated amount deposited. When surface deposition on vegetation is considered the mode of entry, the rate of deposition is of prime importance.

Recently, estimates of the storage time of nuclear debris in the stratosphere have been reduced, indicating that considerable quantities of shorter-lived nuclides are deposited on a world-wide basis, along with strontium-90 and cesium-137 (*4*, *5*). The question to be investigated is whether the concentrations of these nuclides are sufficient to necessitate their evaluation for the assessment of damage to the population.

Monthly activity levels for the fission products strontium-90, cesium-137, cerium-144, zirconium-95, strontium-89, and yttrium-91 are presented in Table 1. These data represent activities found at the time of analysis extrapolated to the end of the sampling month. The levels listed are the averages for a minimum of six collections for each monthly period. Figure 1 illustrates the monthly fallout patterns obtained from the data listed in Table 1. Three peak activity periods are illustrated for each isotope analyzed. The highest deposition occurs in April, with a second high evident in October-November. All isotopes indicate a smaller peak in July, followed generally by a minimum in August and September.

During the first 6 months of 1958, monthly and semimonthly collections were made. The results are reported in Table 2 and show no consistent variation in activity levels between the longer and shorter collection periods. Monthly levels are therefore reported throughout this article.

The half-life values for the nuclides analyzed are as follows (*6*): strontium-90, 28 years; cesium-137, 30 years; strontium-89, 51 days; yttrium-91, 53 days; zirconium-95, 63 days; tungsten-185, 74 days; and cerium-144, 290 days. Because of the long half-lives of strontium-90 and cesium-137 in relation to the 30-day sampling period, their activity levels at the end of the sampling period represent essentially the total amount of activity deposited. However, to obtain total monthly deposition for the individual shorter-lived nuclides, it is necessary to correct for decay during sampling. This is done by assuming a uniform deposition rate during the sampling month. The activity at the end of the month is then corrected mathematically for the effects of both deposition and decay. Since

The authors are staff members of the Health and Safety Laboratory of the U.S. Atomic Energy Commission, New York Operations Office.

fallout is random with respect to time, the net error involved in the year's observations is expected to be small.

Column 2 of Table 3 lists the calculated amounts of the seven nuclides analyzed that were deposited in New York City during 1958. The sum of these activities is 2000 millicuries of beta activity per square mile. Since the total land area of New York City is about 316 square miles, 650 curies were deposited in this area. This corresponds to 200 milligrams of radioactive material. The long-lived fission products, strontium-90 and cesium-137, contributed only 0.9 and 1.0 percent to the total, respectively. Column 3 of Table 3 lists the amount of each nuclide present on 1 January 1959. Totalling these activities we get 690 millicuries per square mile or a 65-percent reduction in beta activity. The contributions of the 12 monthly increments of beta activity to cumulative levels are illustrated in Fig. 2. The effect of radioactive decay on the year's final level (690 mc/mi²) is obscured by heavy fallout during October and November. The contributions of strontium-90 and cesium-137 to the 1 January level were 2.7 and 3.1 percent, respectively.

Table 1. Monthly isotopic activity levels found in New York City fallout during 1958.

Sampling month	Activity level (mc/mi²)						
	Sr^{90}	Cs^{137}	Ce^{144}*	Zr^{95}*	Sr^{89}*	Y^{91}*	W^{185}*
January	1.18	1.08	10.4	27.7		19.5	0.0
February	1.40		14.7	28.7		23.6	0.0
March	1.52	3.22	35.9	93.4		29.7	0.0
April	3.68	3.46	52.4	139	23.6	51.5	0.0
May	3.45	2.95	41.2	49.4	16.7	36.1	2.02
June	1.28	0.57	30.2	32.3	14.3	3.28	10.8
July	1.47	1.98	28.6	52.6	43.9	10.7	47.1
August	0.553	1.48	11.7	21.6	17.1	3.52	31.3
September	0.575	1.23	12.5	22.8	14.3	3.50	24.0
October	1.17	1.82	42.2	114	47.0	10.4	22.0
November	1.19	1.74	35.7	126	36.6	13.5	23.4
December	0.790	1.62	23.8	83.4	25.0	9.34	10.0

* Extrapolated to the end of the sampling month.

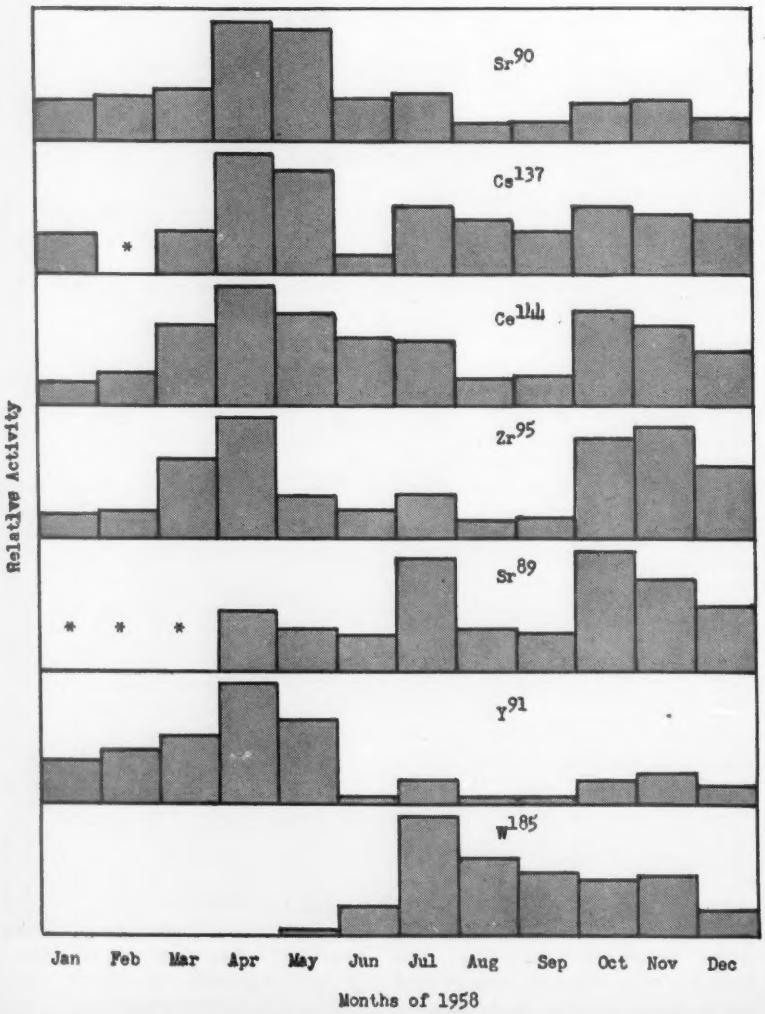

Fig. 1. Monthly fallout patterns in New York City during 1958. Asterisk indicates no data available.

Tungsten Deposition

Tungsten, an element not occurring in fission, was a tracer for the test series conducted by the United States in the Pacific during the spring and summer of 1958. The isotope tungsten-185 was first detected in New York City fallout in the latter part of May 1958. The monthly fallout pattern is illustrated, along with the fission products, in Fig. 1. July was the month of peak deposition of this isotope in the New York City area, and this peak appeared concurrently with the second monthly activity level peak illustrated for the six other nuclides analyzed. From this it appears that the July deposition peak in the New York City area resulted primarily from the Pacific test series, which also contributed to the observed monthly fallout levels for the remainder of 1958.

Solubility of Fallout Nuclides

Strontium-90 and cesium-137 are considered to be biological hazards because of their entry into the food chain and their final uptake by the human body. Martell (*4*, *7*) has studied the chemical state of strontium in fallout debris and reports that strontium-90 is formed primarily as the oxide. Through weathering and absorption of moisture and carbon dioxide, the isotope is generally deposited in long-range fallout as the carbonate.

During the first 6 months of 1958, the solubility of nuclides, as deposited on the ground in New York City, was measured with an adaptation of the collection service described by Welford and Harley (*1*). The unit consisted of two polyethylene ion-exchange columns connected in series and attached to a funnel with a leveling device to prevent either column from running dry during the exposure period. As the fallout is deposited in the funnel, the first column, consisting of paper pulp supported on a glass-wool base, filters out the insoluble material. The soluble material passes through and is collected in the second column, which contains a mixed-bed ion exchanger that adsorbs this soluble material quantitatively. The average percentages of the nuclides found in the insoluble portion (paper pulp) and the soluble portion (ion-exchange resins) are listed in Table 4. From these data it is evident that most of the isotopes are deposited in the New York City area in both soluble and insoluble form. Notable exceptions are the strontium isotopes, which occur predominantly in the soluble form. Cesium-137 in fallout debris is 70-percent soluble, and zirconium-95 is the most insoluble of the nuclides deposited in New York City, showing an average of only 29-percent solubility.

Rainfall

It has been established from strontium-90 and cesium-137 measurements that precipitation is a controlling factor in the deposition of fallout from the lower atmosphere (*7*). Since most of the particles present in long-range nuclear debris are not of sufficient mass for significant deposition to occur through gravitation, deposition is caused mainly by condensation of water droplets around the debris or the adherence of debris particles to raindrops already formed. Therefore, the activity content of the air is also a major factor in the activity levels of fallout deposition. For limited areas, weathered by air masses of reasonably consistent activity concentrations, fallout deposition is proportional to rainfall (*8*).

Over the years, this effect has been obscured in the New York area by large seasonal variations in air concentrations and variations caused by debris dispersed from the Nevada testing site. Figure 3 relates monthly depositions of strontium-90 and cesium-137 to monthly rainfall in New York City during 1958. The dependence of fallout on rainfall is shown by the coincidence of peak precipitation and deposition months in April, July, and October. Stewart *et al.* (*9*) observed that heavy rains lower and light rains increase the specific activity of rain water for individual sites. This was illustrated in New York during September and October, when the monthly precipitation rates exceeded the mean rate for the year and the specific activity was low. In November and December, when the monthly rains were below average, specific activity levels were high.

Since 1955, sharply increased fallout deposition during the spring months has been observed in the northern latitudes (*10*). This phenomenon has been attributed to tropospheric dispersion (*11*) and delayed stratospheric deposition (*12*). Martell (*4*) recently cited fall and winter testing of intermediate megaton devices in the northern latitudes as having temporarily lodged fresh debris in the lower levels of the northern stratosphere. For 1958, large amounts of strontium-89, zirconium-95, cerium-144, and yttrium-91, in addition to strontium-90 and cesium-137, were reported in spring depositions in New York City (Table 1). Figure 3 illustrates the influence exerted by rainfall during this period of increased atmospheric activity. Similar depositions are recorded for March and May, when rain levels were average, but high rainfall in April caused an appreciable rise in the deposition rate. Similar correlation between the deposition of shorter-lived material and rainfall is noted in other periods of consistent, but presumably lower, atmospheric concentra-

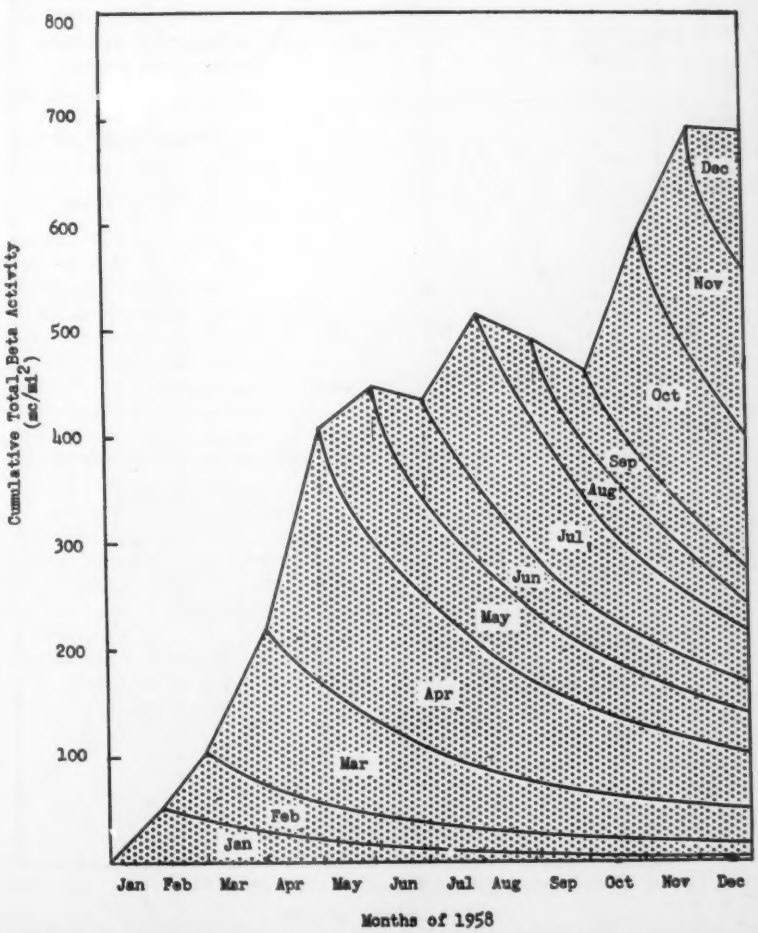

Fig. 2. Contribution of monthly increments to cumulative total beta activity levels in New York during 1958.

Table 2. Comparison of monthly (*a*) and sum of semi-monthly (*b*) activity levels found in New York City fallout during 1958.

Period	Activity level (mc/mi²)					
	Sr^{90}	Cs^{137}	Ce^{144}*	Zr^{95}*	Sr^{89}*	Y^{91}*
			March			
(a)	1.52	3.22	35.9	93.4		29.7
(b)	1.96	3.94	25.2	91.2		47.3
			April			
(a)	3.68	3.46	52.4	139	23.6	51.5
(b)	3.21	4.92	56.4	104	31.3	45.7
			May			
(a)	3.45	2.95	41.2	49.9	16.7	36.1
(b)	3.04	3.49	51.6	88.3	20.8	40.6
			June			
(a)	1.28	0.57	30.2	32.3	14.3	3.28
(b)	1.35	0.89	27.1	67.6	13.7	7.66

* Extrapolated to the end of the sampling month.

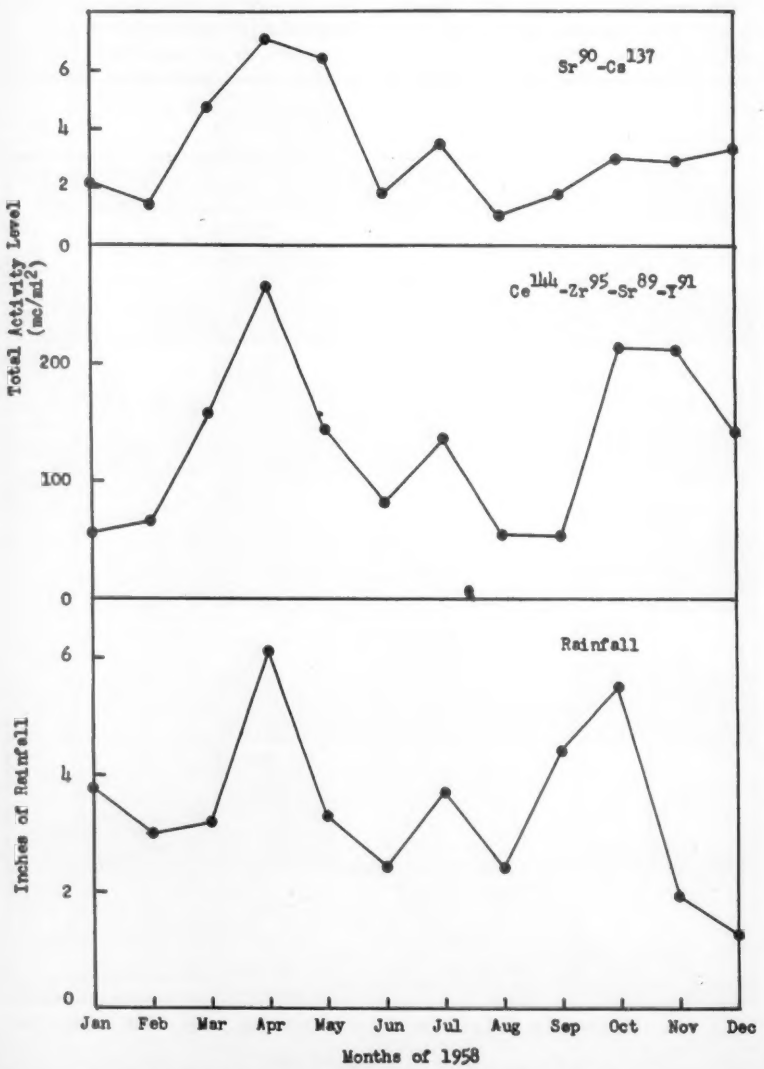

Fig. 3. Comparison of long- and short-lived fallout levels with rainfall in New York during 1958.

tions. July and October (Fig. 1) show simultaneous peaks, and June and August show corresponding minima in the deposition and rainfall curves.

Generalizations on the over-all relationship between fallout and weather are complicated by several sources of error. Sampling, analysis, dry deposition, wind currents, and the eclipsing of short-term phenomena by the extended sampling times may all contribute to the variations appearing in the data. Notable in this respect are the high strontium-90 and cesium-137 activities observed in May (Fig. 3) in conjunction with minimal rainfall and the high short-lived depositions in November, also associated with low precipitation.

Age and Origin of Debris

The approximate age of fallout debris may be established through the relative concentrations of nuclide pairs. Theoretical slow fission yields may be used to approximate the amounts produced by the detonation, and the ratio may be extrapolated by means of the decay constants of two nuclides. Strontium-89 and strontium-90 have been most frequently used in dating long-range debris. Since mixtures of old and fresh debris result in enrichment of strontium-90 with respect to strontium-89, ratios of shorter-lived materials have proved more sensitive.

During 1958, peak depositions of shorter-lived nuclides in New York in April, July, and October–November indicated the arrival of debris from several recent test series. To determine the age of the monthly depositions, Sr^{89}/Sr^{90} and Zr^{95}/Ce^{144} ratios were used. Table 5 lists apparent production dates calculated from these ratios. There is generally good agreement in the two approximations. The debris collected from January through June originated primarily from tests occurring in the fall of 1957. In March and April, debris from the tests conducted in the winter months of 1958 arrived along with older debris. Collections for July through December have already been associated with the U.S. series in the spring of 1958, in which tungsten-185 was produced. The nuclide ratios corroborate this evidence and also show the arrival from October through December of debris from the U.S. and Russian fall tests.

Table 3. Cumulative aspects of New York City fallout during 1958.

Nuclide	Total beta activity deposited during 1958 (mc/mi²)	Calculated total beta activity on 1 Jan. 1959 (mc/mi²)
Sr^{90}	18.2	18.2
Cs^{137}	21.2	21.2
Ce^{144}	349	173
Zr^{95}	925	277
Sr^{89}	294	93.2
Y^{91}	264	33.4
W^{185}	192	71.6

Nuclide ratio approximation is limited by several factors, including variation of actual yields from the theoretical and fractionation of nuclides in the fireball of the detonation. These limitations are compounded, in the dating of long-range fallout, by mixture of debris before deposition. This is especially true of strontium-90, which outlasts strontium-89 in old debris and distorts the dating of mixtures. This effect is observed in April and May samples (Table 5), where the strontium internal ratios yield earlier apparent production dates than the Zr^{95}/Ce^{144} ratios.

Table 4. Solubility of nuclides in fallout.

Nuclide	Percent insoluble (av.)	Percent soluble (av.)
Sr^{90}	0.03	95.6
Sr^{89}	3.56	94.4
Cs^{137}	22.6	70.0
Y^{91}	47.6	52.4
W^{185}	52.3	45.6
Ce^{144}	57.3	42.0
Zr^{95}	67.8	29.1

Table 5. Age of debris estimated from nuclide ratios in New York City fallout during 1958.

Sampling month	Apparent production dates	
	Zr^{95}/Ce^{144}	Sr^{89}/Sr^{90}
Jan.	Nov. 1957	
Feb.	Nov. 1957	
Mar.	Jan. 1958	
Apr.	Feb. 1958	Sept. 1957
May	Nov. 1957	Sept. 1957
June	Dec. 1957	Jan. 1958
July	Mar. 1958	Apr. 1958
Aug.	Apr. 1958	May 1958
Sept.	May 1958	May 1958
Oct.	Aug. 1958	Aug. 1958
Nov.	Oct. 1958	Aug. 1958
Dec.	Nov. 1958	Sept. 1958

Conclusions

Rainfall is clearly a controlling factor in fallout deposition patterns. Proportionality relationships between activity and rainfall, which have been established for areas of consistent atmospheric activity, pertain also to other areas during limited periods when air concentrations are static. The dependence of fallout on rainfall is not notably different for old and fresh debris or for long- and short-lived nuclides.

Strontium-90 and cesium-137, biological hazards in fallout material because of their similarity to calcium and potassium, respectively, are deposited in predominantly soluble form, and this increases the possibility of entry into the food chain.

The deposition pattern of tungsten-185 in New York City during 1958 shows that nuclear tests contribute to off-site fallout within weeks of the detonation. Moreover, this contribution is measurable for at least 8 months, even when testing and sampling occur in widely separated locations.

The approximate age of the debris collected in New York during 1958 is established through fission-product ratios. When an average age of 100 days is assumed, it is estimated that the seven nuclides measured account for about 40 percent of the year's total beta activity. When the calculated amounts of yttrium-90, praseodymium-144, niobium-95, ruthenium-103, cerium-141, barium-140, lanthanum-140, ruthenium-106, rhodium-106, and promethium-147 are added to values for the nuclides measured, it appears that the total deposition of beta activity in New York City during 1958 was 5 curies per square mile, and that the level on 1 January 1959 was 1.7 curies per square mile. Less than 3 percent of either of these values is attributable to the Sr^{90}-Y^{90} and Cs^{137}-Ba^{137} chains.

According to prevailing fallout distribution theory, New York fallout levels are representative of a mid-latitude band covering about 15 percent of the earth's surface. Since the fallout in this area is no more than four times that of other areas, it is obvious that continued testing at a rate similar to, or above, that in 1958 will necessitate the complete documentation of deposition levels for shorter-lived nuclides. Any evaluation of radiation dose to the population from fallout must include these activity levels (*13*).

References and Notes

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Knowledge: a Growth Process

Knowledge grows like organisms, with data serving as food to be assimilated, rather than merely stored.

Paul Weiss

It seems appropriate for a member of the American Philosophical Society for Promoting Useful Knowledge to review for himself on occasion the philosophy by which useful knowledge is promoted. I have indulged in such a private exercise, not as a philosopher, which I am not, but as a practitioner of science. In our day, promotion of knowledge has become a public trust. Its managers and practitioners must see and keep the object—knowledge—in sharpest focus, lest it get blurred in the excitement of a mass-production boom, or get disjointed by progressive parceling among producers, sponsors, distributors, interpreters, administrators, and consumers.

Promoting knowledge can only mean fostering its intrinsic growth. To do this rationally requires insight into the nature of the growth process. I wish to show that, fundamentally, our knowledge grows the way a living body does. The kind of knowledge I shall deal with is scientific knowledge, implying no inference to other forms. And even this limited perspective is slanted from the angle of my specialty, the life sciences. Yet, it takes the vantage point of a biologist to recognize the growth of knowledge as truly a mirror image of the growth of organisms. Not long ago, I summed it up as follows: "Scientific knowledge grows like an organic tree, not as a compilation of collector's items. Facts, observations, discoveries, as items, are but the nutrients on which the tree of knowledge feeds, and not until they have been thoroughly absorbed and assimilated, have they truly enlarged the body of knowledge" (*1*). This thesis I shall now try to expound.

The author is member and professor at the Rockefeller Institute, New York. This article is based on a paper read at the meeting of the American Philosophical Society, Philadelphia, 13 November 1959, and is being printed simultaneously in the *Proceedings of the American Philosophical Society* **104**, No. 2 (1960).

My model is a higher animal. The main steps of its growth process are diagrammed in the upper half of Fig. 1, with boxes indicating material entities, and arrows, the flow of processes connecting them. Growth converts food from the environment into body substance in a sequence of four major steps: intake, digestion, assimilation, and final utilization. The raw materials are gathered from the environment and either stored or passed on directly for alimentary processing. Digestible items are broken down chemically to more manageable compounds, which are then screened and sorted into useful and useless varieties. The wastes, together with undigestible residues, are eliminated, sharing the fate of spoilage from protracted storage. The useful items, the true nutrients, are circulated to the tissues, whose cells pick what they need, then recombine and modify it to form intermediary products, already bearing specific earmarks of that organism, some to be recirculated for use by other cells, some still to be discharged as waste; and finally, culminating the synthesis, each cell constructs from this supply pool selectively the substances and structures uniquely characteristic of its own kind. In this last step, cells branch in two directions: They either reproduce, that is, add more cells to the body; or they turn to the manufacture of special products, like fibers, hair, secretions, bone, and such.

This model is abridged and oversimplified. However, it illustrates the essence of the growth process, which is that in its growth an organism never adopts foreign matter outright, but reorganizes and assimilates it to fit its own peculiar pattern. Even a leech must first dissolve the hemoglobin of its meal of blood and then compose its own brand from the fragments. Organic growth is by assimilation, not accretion. Food items are not simply stuck on to the body, but, on the contrary, lose their identity and become anonymous and indistinguishably blended into the body's very own type of constituents by the processing chain of extraction, screening, sorting, fitting, and recasting.

How closely this course is mirrored in the growth of knowledge is symbolized in the lower half of Fig. 1, beginning from its source—experience, still unprocessed. Probing of the environment furnishes the raw data of information, which are either stored as records for future use or analyzed forthwith. The products of analysis are then screened and sorted according to relevance. Irrelevant ones go into discard, sharing the fate of records become obsolete. And from this sorting, the pile of data emerges as an ordered system, catalogued and classified, yet each item still revealing its erstwhile identity. The grandest examples of such ordered sets of data are perhaps the Linnean system of species prior to the theory of evolution, or the Mendelyeev Atomic tables prior to modern physics. In various stages of evaluation, such packaged information is then widely circulated, leading to confluence and critical correlation with countless contributions from other sources. From this synthetic process, hypotheses emerge, which, upon further verification, turn into integral parts of the body of knowledge—theorems, principles, rules, and laws—general formulas which not only supersede the itemized accounts of the very data from which they were derived, but can dispense with the further search for items of information, which they predictively subsume.

At this stage, data have become assimilated, have lost their individual identities in merging with that higher entity—the body of organized knowledge. Sheer listing has given way to understanding. A patchwork of unrelated facts has been transformed into a rationally connected thought structure of inner consistency, viable and durable, subject to the tests of survival and the adaptive improvements of evolution—a veritable model of an organism. As in the organism, the culminating phase is branched: as basic knowledge grows, part of the increment accrues to its own body, yielding more basic knowledge, while another part is converted into differentiated products—all that is commonly lumped under the attribute "applied."

Note that no separate express tracks connect either foodstuffs directly with

functional products in the organism, or informational data with practical results in human affairs, but that both must be routed through the common machinery for growth. In knowledge, as in nature, fruit grows on trees and cannot be raised directly on the soil by short cuts bypassing the tree.

Our growth analogy could be expanded—tradition standing for heredity; novel ideas, for mutations; the "team" approach, for symbiosis—but the general parallelism will have become clear enough for us now to examine its implications.

In the first place, it shows that information is not tantamount to knowledge. Information is but the raw material, the precursor, of knowledge. To hoard a store of unrelated items of information in a mental gullet by rote memory and without sense of relevance —including the ability to regurgitate the data on a quiz master's prompting—should pass for knowledge no more than the stuffing of a hamster's pouch can be regarded as growth. Knowledge emerges from the distilling, shaping, and integrating of the raw material into concepts and rules; and in this process of condensation and generalization, the number of bits of detailed information dwindles, rather than mounts: a piling up of raw data signals glut rather than growth.

Accordingly, if knowledge grows like organisms, we ought to observe sound dietetics and avoid unhealthy overstuffing; the symptoms of glut—redundancy, superdetermination, oversophistication, and just plain bulk—are already noticeable in current research practices. Part of the syndrome carries rather undignified names, such as "soft money" or "projectitis." But the crucial ailment is myopic vision, which fails to recognize the true character of knowledge. Once out of sight, the body of concepts to which data collection should be related no longer guides the search for data. The sense of relevance and selectivity becomes atrophic, composing stops at sheer compiling, search becomes pointless, and freedom of investigation degenerates into license for random movements.

The diagnosis calls for preventive therapy. One nostrum proposes that research be governed, with social utility as the beacon. Unfortunately, the diet this prescribes for knowledge is of the sort that social insects feed to their larvae to mold them to preordained stations in life, mostly soldiers and sterile workers, instead of nurturing versatile and reproductive specimens. By contrast, I submit that knowledge grows best on a liberal balanced diet based on variety and wide freedom of choice, free of excessive roughage.

Now, who is there to write the formula? We all abhor the notion of an all-wise potentate of knowledge, whether person, institution, or society, to rule on what will, and what will not, promote the growth of knowledge. But there is one wizard, who has the formula and gladly hands it over for the

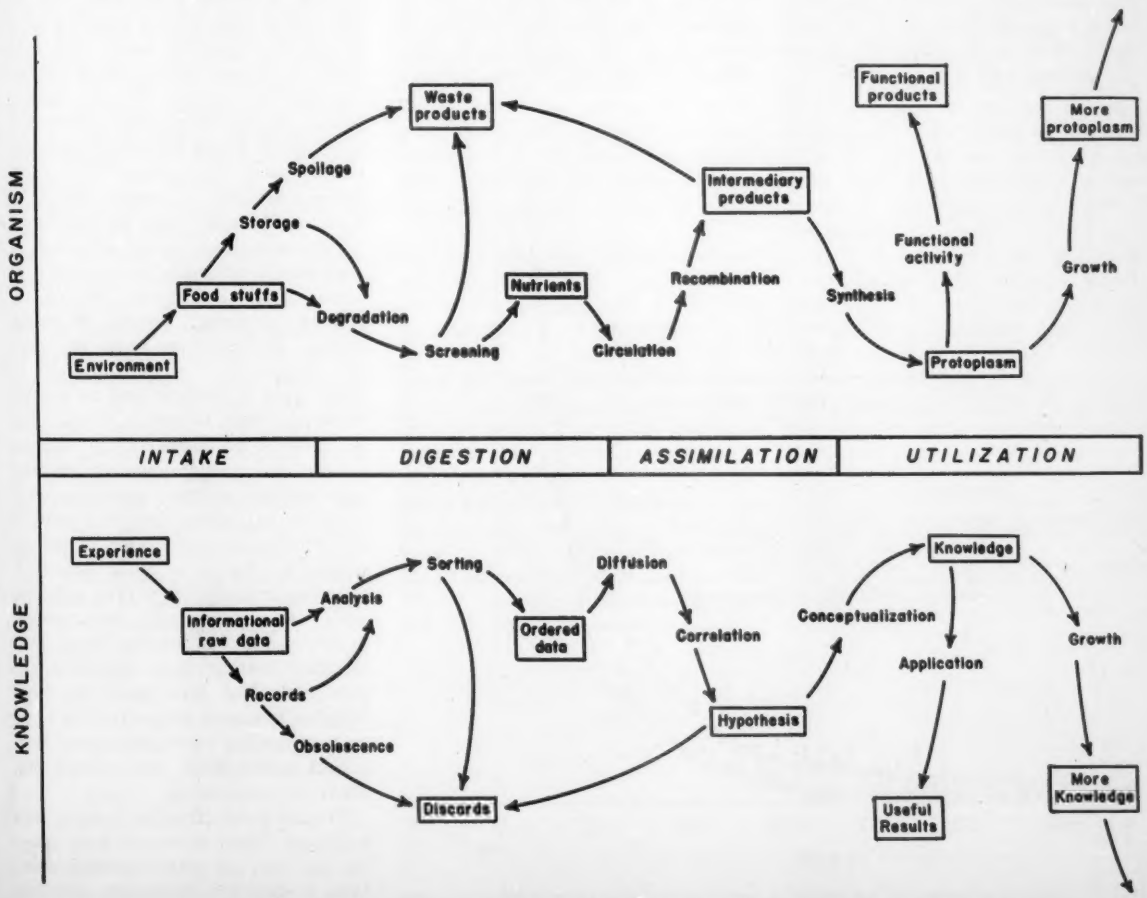

Fig. 1. (Top) The growth process of higher animals. (Bottom) The growth of knowledge.

asking—history, which has watched knowledge grow from infancy. It tells us that the key agent in the growth of knowledge has always been the human mind, imaginative, critical, and integrative; devising robots, tools, and techniques merely as aids to extend the limited reach of man's perception and control, but never abdicating the functions of evaluation and invention. Promoting knowledge, therefore, implies giving full scope to the exercise of the faculty for assimilation and synthesis by which the mind converts facts to knowledge. The history of knowledge contains the rules to give those bent on promoting knowledge by either doing or supporting research the necessary cues for intelligent and responsible self-direction. The stress here lies on "self"; there is no need for forcible external steering: bearings for self-steering are needed. But how could the uninitiated self orient itself purposefully if we hide or blur the goal? Or do we expect each self to rediscover the goal for himself by trial-and-error, fumbling and floundering in semidarkness, when we could readily draw on the lessons of the past to illuminate both goal and path?

You realize what I am driving at. As educators we mold tomorrow's promoters of knowledge. We must be far more explicit than we have been lately in teaching them not just the present state of knowledge, but the way in which it has grown up to here, which is the only way in which it can grow further. Inspired teachers teach and practice it, but they are too few. Some students find it by themselves, but not enough of them. So, let us restore to education some fundamentalism—making explicit to the student the fundamental bearings needed for him to chart his own course in clear view of what furthers knowledge and what does not, instead of letting him drift in the cross currents of traditional momenta and alluring fashions. Ideas—yes, even well-founded speculation—should find a respectful place again among the shining gadgets. And his critical mind, rather than the board room of a fellowship or grants committee, should become again the primary testing and screening ground for relevance. If he finds data, let him explain their meaning. And if he can't he should have a sense of incompleteness, and not of glee over having prevented mental contamination of nature. Let editors encourage, rather than blue pencil, an author's interpretive excursions. And let the whole process of fostering knowledge become refocussed on penetration and concentration, instead of sheer expansion and bulk. Or else, knowledge—an organism—might come to share the fate of the dinosaurs.

Yet, notwithstanding this plea for more thorough digestion and mental processing of data, there is another side of nature which is refractory to this treatment and does not fit our analog at all. I am referring to those phenomena whose constellation in space or seriation in time is so unique that generalization would obliterate their most relevant features. We can establish general principles of parasitism, but each species of parasite has its own peculiar life history, which must be known as such. Chemical chain reactions must conform to thermodynamic law. But just what sequence of steps constitutes a given metabolic cycle must still be determined separately in each instance. Despite their common name, each hormone has its own special way of operating, and each disease has its specific course. In other words, the information which in the search for basic knowledge would merely be a way station, becomes a terminal; to remain useful, its itemized character must be preserved.

This seems the proper province of automation, relegating increasingly to technological devices the jobs of recording, scanning, sorting, reducing, storing, and retrieving data. But even here human intelligence will have to judge what to explore and to record. As every single walnut is unique, can we afford to go on indefinitely shelling walnuts and loading down our libraries with records of their physiognomies? Certainly not, unless there is a point to it. To make the point is up to the investigator. But it is up to educators to imbue investigators with a sufficient sense of relevance and responsibility as to abstain from pointless tasks. Self-direction must not be let lapse into self-indulgence. But how draw the line? Whether to stop at pragmatism or to go on to generalize varies, of course, with subject matter, need, and interest but, above all, perspective.

To gain perspective, let us again turn to history. What is—so we may ask—the real fate of plain recorded data? What is their life expectancy, and does it differ substantially for data that can

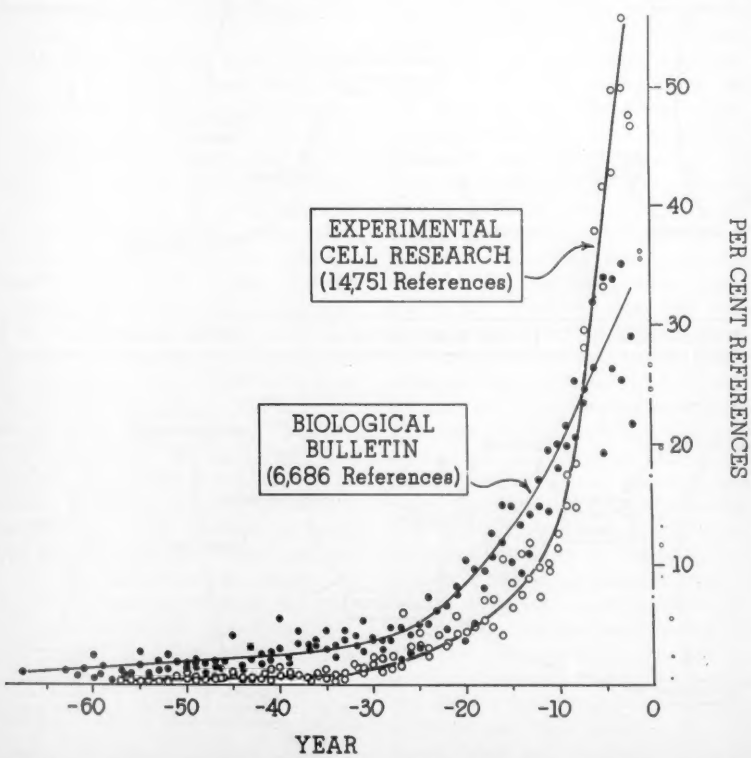

Fig. 2. Annual percentages of references to works published a given number of years previously.

be generalized and those that cannot? Since mere occupancy of library space is no criterion of life, as against mummification, I made a little actuarial study, using as a gauge of relative vitality of data the frequency with which they are referred to in the literature.

For comparison, I chose two biological journals: *Experimental Cell Research*, with a strongly analytical emphasis, and the *Biological Bulletin*, with a larger descriptive bias. I tallied all references, except self-citations, by all authors year by year over a 10-year period and plotted the percentage frequency with which publications were cited lying back 5, 10, 15, 20, and so forth, years. Figure 2 shows the results, which are quite striking (*2*).

For each of the two journals, the annual percentage frequencies define with remarkable consistency a single curve, whose course expresses the rate of obsolescence of publications. The steepness of its slope should give us pause. No more than 50 per cent of the annual references in *Experimental Cell Research* date further back than five years, and still older literature is rapidly lost sight of. By contrast, the flatter curve for *Biological Bulletin* reveals a much greater dependence on older records. The difference is highly significant. To validate it further, I plotted the chronological frequencies of back citations during 1952–1954 in the major journals in physiology and in zoology and entomology (Fig. 3) from a report on "Scientific Serials" (*3*), and obtained an essentially similar pair of curves, both dropping off sharply, but the drop being much steeper in analytical physiology than in its more descriptive biological sister sciences.

The lessons of this actuarial census of literature thus are twofold: In the first place, the active life span of pure data is at any rate amazingly short: they die of either assimilation or oblivion. And second, the less they lend themselves to assimilation, the longer they remain useful individually.

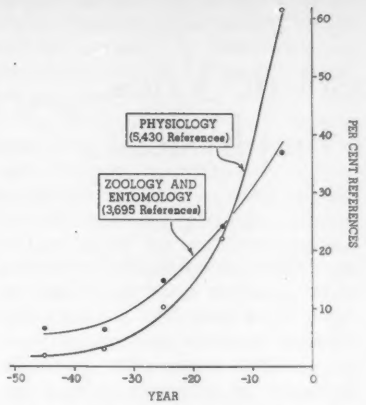

Fig. 3. Percentages of references, in the major journals in physiology and in zoology and entomology, referring to work published a given number of years previously.

This leads me to conclude as follows: Each field of knowledge must be accorded its own merit ratio between generalization and particularization, it being taken for granted that assimilation will be driven to the utmost limits compatible with the nature of the field. Yet, in view of the rapid attrition of *all* unassimilated information, a radical reorientation of our publication policies would seem warranted, based on introducing the principle of actuarial tables, about as follows:

In each discipline separate media of publication would be established for classes of communications of different life expectancies. Each author of a manuscript would assess its prospective useful life span—presumably with the benevolent advice of editors—on a rating scale extending from the ephemeral technical note at the low end to the great synthetic opus at the other; the paper would then be allocated to the corresponding fast-aging or slow-aging class of serial. Each serial volume would be kept on library shelves only for the time span allotted to its class, and then discarded, except in a few libraries specifically designated as permanent historical repositories. Some such deliberate scheme would go far in restoring and preserving a reasonable ratio of payload to ballast in our records of knowledge.

Graded in terms of relevance, not every observation is worth reporting; not every report is worth recording; not every record is worth publishing; and not every publication is worth preserving for eternity, except in sample specimens as in Noah's Ark. I submit that this grading can still be left to the investigators and their peers, as long as they are cognizant of the true nature of knowledge as a growth process, of which assembling facts—of food for thought—is but the first preliminary step; a growth process, moreover, which often thrives better on a spare than on an overly rich diet, and in which self-restraint can readily ward off obesity.

So in conclusion, and dropping parabolic language, the effective pursuit of knowledge is intimately linked to the old virtue of disciplined research morale which will not countenance the substitution of bigness for greatness, gadgets for intellect, projects for ideas, and man-hours for thought; although it must rely to the fullest on technical relief by gadgets and man-hours in those auxiliary services which do not require the intervention of a constructive mind.

As I said at the outset, my comments are confined to *scientific* knowledge. Its steady long-range growth has still immense potential scope. We should not let it get diverted, inflated, and unbalanced by pressures for short-range crash spectaculars. More than ever, our key words should be *balance* and *perspective*.

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Hessel de Vries, Physicist and Biophysicist

On 23 December 1959 death came to Hessel de Vries, at the age of 43. De Vries was professor of physics at the University of Groningen and a member of the Royal Dutch Academy of Science. Not only in the realm of physics but to all of science and to all scientists his death is an almost irreparable loss.

His scientific career lasted little more than 20 years. During that time he made significant contributions to divergent fields of natural science. Once his interest in a problem was aroused, he did not rest before he had thoroughly investigated all its consequences; less essential matters had to wait until a solution had been found and a deeper insight had been reached.

It is impossible to give here a survey of his work which would be in any way complete. In reading through his more than 80 publications, one's attention is repeatedly drawn to subjects which are but loosely connected with the main fields of his research. I will therefore restrict myself to pointing out some of the results of his work in the three principal fields in which he became particularly distinguished: neutron physics, the area of his doctoral thesis; biophysics, in which he received a chair in 1954; and carbon-14 research, which he had fully mastered and which he developed in Groningen to a degree of perfection unequaled anywhere.

During the period in which de Vries conducted his research in nuclear physics (1938–1946), information about the interaction of nuclei with neutrons was scarce and not very reliable. In particular, measurements of resonance parameters produced doubtful results at a time when the time-of-flight method had not yet been developed. The filter methods for measuring resonance widths, published by de Vries in 1942, resulted in substantial improvement in this field.

In 1943 de Vries had already become interested in the biophysics of the sensory organs, and at this time he published an article on the influence of the quantum character of light on vision. Some years later this work was extended to include an investigation of color vision; among other things he developed an ingenious method for independently measuring the sensitivity curves of the blue, green, and red receptors. With this method, different kinds of color blindness could clearly be distinguished. In particular his measurements of the sensitivity curve of the red receptors in the long-wave region became widely known. He explained the form of this curve by assuming that the thermal motion of the molecules supplies the energy that the quanta of red light lack for stimulating the red receptors. A confirmation of this theory was found in the "red shift" of the sensitivity curve, occurring when the temperature of the eye is raised. Anyone working in Groningen at that time remembers these experiments, where the test subject was placed in a transformer vessel filled with hot water. This water was kept at the right temperature by Bunsen burners.

Hearing also became a subject of de Vries' investigations. These studies embraced all functions of the ear, especially the transport of sound within the auditory passages. Involved, too, were studies of the static and dynamic sense of equilibrium and of the microphonic activity of the semicircular canals and the cochlea. When his knowledge of the anatomy of the inner ear proved insufficient, de Vries applied himself to detailed study. Together with Kuiper he made extensive measurements of the microphonic activity of the lateral line organ of fishes. Here, for instance, an explanation was found for the rectification occurring in the cupula.

Hessel de Vries

In the course of his physical research on the senses de Vries was often able to demonstrate, in a surprisingly quantitative way, to what extent nature had succeeded in optimal construction of the organ. In particular, threshold values of sensitivity often proved to be such that Brownian motion could have no adverse influence.

In his biophysical work de Vries learned not only the attractions but also the pitfalls which nature has provided for those who want to force her into the mold of a physical model. Sometimes his theories proved to be wrong; he acknowledged this once with the words: "A theory may be wrong, but that does not matter; it is more important whether it is usable, whether it widens our scope and incites new experiments." A survey, with many references, of his biophysical work may be found in *Progress in Biophysics* [H. de Vries, "Physical aspects of the sense organs," *Progr. in Biophys. and Biophys. Chem.* **6** (1956)].

How important physical methods and a physicist's point of view can be for research in fields not originally of a physical nature was again and even more impressively demonstrated by de Vries in his application of the carbon-14 method to archeological, geological, and climatological problems. Since there exist excellent surveys of the carbon-14 method [for example, H. de Vries, *Ned. Tijdschr. Natuurk.* **23**, 277 (1957)], I shall only mention here how he succeeded in improving this method to such a degree that at Groningen it is now possible to determine ages up to 50,000 years with small quantities of carbon, and up to 70,000 years when the samples can be enriched in the C^{14} isotope.

The important improvements of the carbon-14 method made by de Vries resulted from strenuous labor, guided by his thorough understanding of the physics of the method. In the first place

he discovered that carbon dioxide can be used as a gas in a proportional counter, provided it is extremely pure. Carbon dioxide counters are now used in almost every carbon-14 laboratory. Their greatest advantages over counters with solid carbon are their much higher efficiency, which makes possible the use of much smaller samples, and the fact that they can be filled easily and accurately. Further, de Vries developed methods of purifying the gas in little more than one hour. His thorough analysis of the background of the counter led to effective measures for its reduction and to a correction for fluctuations in its intensity. Much attention was always given to the chemical pre-treatment of the samples. Especially in old samples, the possibility of contamination with recent carbon was always carefully investigated. Very often de Vries went to dig out his samples himself, even to North America, in order to be absolutely certain that the samples were not, through injudicious treatment, contaminated by recent carbon.

De Vries contributed to many investigations of archeologists and geologists, not only by dating their samples but also by actively engaging in their research. His own particular interest, of late, lay in the chronology of the climate of the last glacial period. He published many new facts about this in the last year of his life.

Besides his study of the "macro"-climate, de Vries discovered a remarkable correlation between "micro"-climate (that is, temperature fluctuations within a period of about 100 years) and fluctuations of ± 1 percent, with respect to the average, in the carbon-14 activity of the last 400 years. Although this made many datings of more recent samples less reliable, on the other hand it did explain some serious discrepancies.

De Vries often saw his scientific work as a game, but he played this game with the utmost concentration, its rules being the laws of nature. He used these laws in a way which clearly demonstrated how they had become a part of him. His ways of dealing with physical problems provided an example worth more than many a neatly prepared lecture. He disliked all ostentation, to such a degree that in lectures he often glossed over his own achievements. His untimely death is a great personal loss to many scientists all over the world.

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Science in the News

Radiation Standards: Testimony at Congressional Hearings Tends To Be Reassuring

Despite some differences in emphasis there appeared to be a broad agreement among the several dozen scientists who appeared at the radiation hazard hearings ending last week that the risks involved at present levels of exposure are quite small compared either with other hazards of modern life (cigarettes, air and water pollution, and automobile accidents, to name three) or with the benefits derived from the use of radiation.

The testimony, before the Joint Committee on Atomic Energy, emphasized that there are substantial uncertainties about the amount of harm, if any, that is likely to result from the low dose rates to which people are now being exposed. Because of this the witnesses agreed that it was prudent to take steps to see that exposure is kept as far below the recommended "guidance levels" as practical. There were differences of opinion over just how much concern is justified over the probable damage stemming from current exposure rates, but none so sharp as to lead anyone to suggest that the Radiation Protection Guides proposed last month by the Federal Radiation Council need to be revised downward. These guides, it was repeatedly pointed out, do not represent danger points but only control points below which the likelihood of any individual being harmed is believed to be so small that any reasonable increased use of radiation that promises some benefit should be permitted. The current level of exposure for the general population was estimated to amount to 10 percent or less of these guidance levels, and there appeared to be little likelihood that the general level of man-made radiation would climb near the over-all guidance levels in the foreseeable future.

The guides apply to all man-made radiation, including fallout, with the exception of medical uses. These medical uses were said to account for 90 percent or more of the exposure to man-made radiation, but since their contribution to the health of the public far outweighs the most pessimistic estimates of the incidental damage from them they cannot be considered a health hazard in the sense that other exposure to radiation is so considered. In any event, whether to take a given x-ray was felt to be a matter to be decided by the physician handling the individual case. But it was pointed out that improvements in x-ray technique and equipment could probably reduce the incidental hazard for both the physician and patient by 10 percent, and perhaps considerably more, without in any way limiting the benefits. A reduction of this size would probably be equivalent to the complete elimination of all other man-made radiation.

Radiation Protection Guides

Based on statements such as the report of the National Committee on Radiation Protection which appeared in the 19 February issue of *Science*, the method for calculating the radiation guides appears to be this: You assume, first, that there is no threshold level below which radiation is harmless; second, that the probable effects are directly proportionate to the dose; and third, that the damage from chronic exposure to low dose rates will be the same as for a dose of the same total size accumulated at a high rate in a short

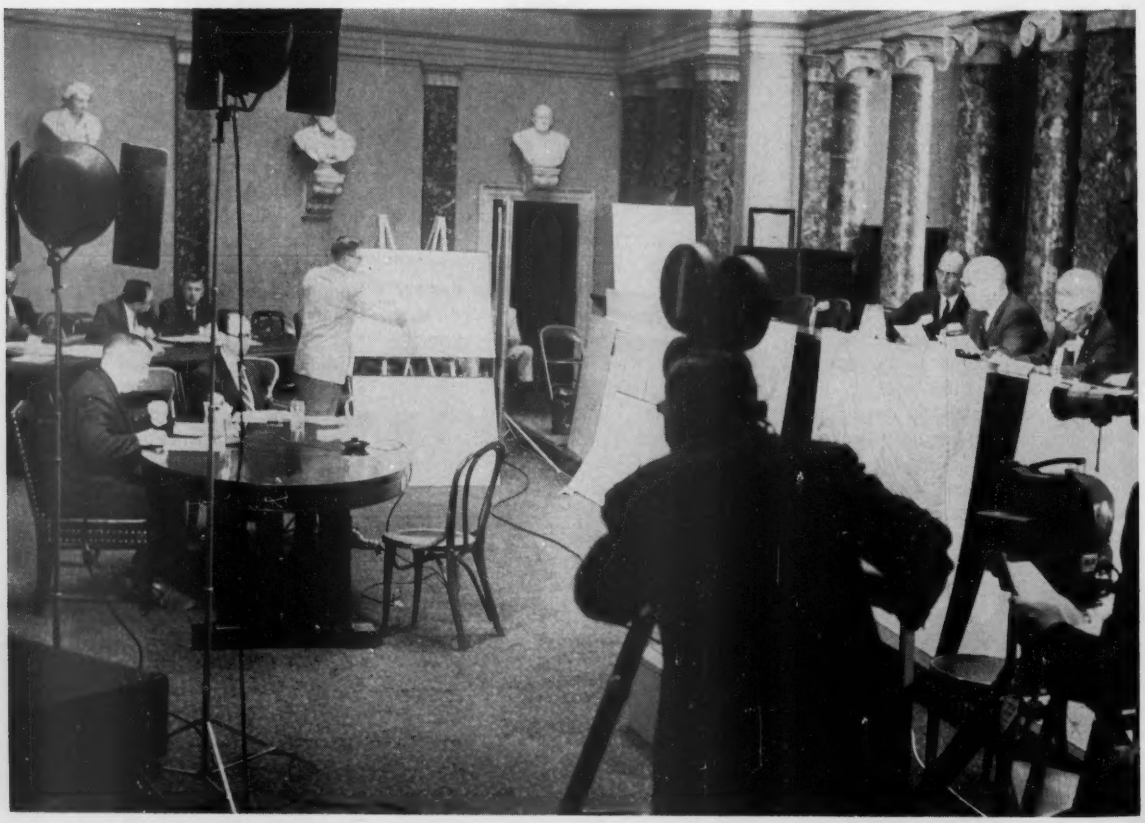
time. All of these are considered to be the most conservative reasonable positions. You then extrapolate conservatively from the available data, all of it based on high dose rates or acute doses, since studies of groups exposed to comparatively low rates have not yet turned up any clear evidence of damage. You then try to calculate a dose rate where it will be very unlikely that any given individual exposed to such a rate over a period of many years will suffer any discernible damage. You then, apparently, take this figure and compare it with the state of technology. If it is high compared to the limits of radiation that can be demanded without stunting the growth of beneficial uses of radiation, you lower it some more to add an additional margin of safety and because you do not want to encourage laxness by setting the standard so high above current levels that people are led to think there is nothing at all to worry about. You thus arrive at a number which you set as the control point for radiation workers.

To set a guide for the population at large, you divide this number by ten, partly because you do not want to expose the general population to as much risk, however low, as radiation workers are exposed to, but more importantly because there is evidence to indicate that children are more sensitive to radiation than adults, and, in any case, they have more time prior to parenthood to accumulate a genetically significant dose. All of this, much simplified, is how the guides seems to be set, and why the scientists who set them seem to be concerned lest they be misinterpreted as absolute limits which must under no circumstances be exceeded.

Problem of Research

Nearly everyone who appeared before the committee had some suggestions for further research. The difficulty was that while everyone agreed that he would like to have more precise information on the risks involved at various levels of exposure, it was also agreed that most of the desired information could be obtained only through extremely elaborate statistical studies which tend to be expensive and, unless carried out with great care and competence, of questionable value. The reference to expense, it was made clear, was not meant to be merely in terms of dollars, but also in terms of making the best use of the limited scientific talent available for this work.

The difficulty, it was pointed out, is that if you are looking for an effect that is probably going to show up only a few times in a million cases, you obviously must work with very large numbers merely to bring your probable error down below the level of your expected deviation. The committee was told, for example, that the U.N. team studying the genetic effects of the high level of natural radioactivity in Kerala, India, is doubtful whether they will come up with any clear statistical evidence of an increase in the mutation rate, despite the fact that they are working with a sample of 80,000 people whose ancestors for many generations



The Joint Committee on Atomic Energy usually holds its hearings in the Old Supreme Court Chamber in the Capitol. This photograph was taken during the earlier hearings on nuclear warfare.

have been exposed to a level of radiation of about 10 times as high as the rest of the race.

It was repeatedly pointed out that, contrary to a widespread feeling among the public, there is nothing unique about the effects of radioactivity. All of the diseases and the genetic effects known to be associated with radiation are also known or suspected to be caused by natural and by man-made contaminants as well. There is no way to tell whether any specific case of leukemia, for example, was caused by radiation. You can only try to find a meaningful correlation between exposure to radiation and some statistical increase in the effect.

Those difficulties put the scientists working in this field in an awkward position. There seems to be a substantial segment of opinion that assumes that because one group of scientists can tell you what the temperature is on a star billions of miles away that the biologists must not be doing their job very well if they can't tell you exactly how many cases of X diseases are going to result from Y amount of radiation absorbed over a 20- or 30-year period.

Federal Radiation Council

The organization officially charged with formulating a national policy on radiation is the Federal Radiation Council, but the hearings made it clear the FRC, now a year old, has yet to assume any really significant function. Its membership currently consists of the Secretaries of Labor, Commerce, and Defense, the chairman of the Atomic Energy Commission, and the Secretary of Health, Education and Welfare. But its permanent full-time staff consists of the executive secretary, and the secretary to the executive secretary. Ad hoc committees are named to study the problems that come before the council.

Its principal accomplishment to date has been to replace the term Maximum Permissible Dose with the term Radiation Protection Guide. The change is not as trivial as it may seem and may turn out to be very useful. The older term was unfortunate both because "maximum" suggested a limit that could never be exceeded without getting into an area of gross danger and because "permissible" suggested that there was no need to pay much attention to radiation below the "permissible" level. In fact, it was agreed both that radiation exposure should be kept as far below the permissible levels as possible and, at the same time, that if circumstances demanded, an individual could be exposed to considerably more than the maximum level without exposing him to any more danger than is encountered much more frequently and with much less concern in other fields of work.

Beyond this change in terms the council has yet to do much of significance. It has prepared a directive, which was issued last month over the signature of the President, setting standards for the various government agencies to use. But the standards are essentially identical with those issued by the National Committee on Radiation Protection. These NCRP standards were already accepted by the AEC, the Defense Department, and other agencies. The FRC directive merely serves to make them official rather than semiofficial. It appeared, in fact, that the FRC was formed partly out of a general feeling that the government ought to "do something" about a problem that was of general concern, and partly to resolve a jurisdictional dispute over who should set national policy between the AEC, which has most of the experience, and the Public Health Service, which feels it should have the responsibility. A need was also felt for separating the responsibility for promoting the use of atomic energy from the responsibility of preventing any development that might lead to unwarranted hazards. At present both functions are largely in the hands of the AEC.

The Joint Committee was dissatisfied with the FRC's present situation, and suggested a number of things the council could do to make itself more useful. The committee wanted the FRC to formulate clearer answers than it now appears to have to the questions of: who has the actual responsibility for determining whether any proposed increased use of radioactive materials, including anything planned by the Defense Department, will lead to benefits commensurate with the rise in radiation levels that may result, and what criteria are going to be used to make these decisions? The committee also seemed to feel that if, as it now appears, the principal function of the FRC is to reassure the public that its interest and well-being are being looked after, that its usefulness would be increased by including representatives of labor, management, and other public interests on the council.

Yet despite the questions raised, the general effect of the testimony was reassuring. Even most of the uncertainties, it was clear, were reflections of the precautions being taken to prevent radiation from developing into a major hazard rather than indications of danger. Much more is known, the committee was told, about the hazards of radiation than about any number of other sources of contamination produced by modern society, and much stricter steps are being taken to control the hazard.

Food Additives Law Nears Passage

The House Commerce Committee has approved legislation that includes the "Delaney clause " flatly barring the use in foods or cosmetics of any coloring matter that can produce cancer in man or experimental animals. The bill is expected to pass the House without difficulty, if only because it would take a very brave legislator to take a position in an election year that could be interpreted by his home district opponents as a vote in favor of cancer. A Senate color additives bill went through without the Delaney clause, and the issue will have to be settled in conference.

The issue has been a controversial one, even within the Administration. The Department of Health, Education, and Welfare has favored the clause on the grounds that the existence of a threshold dose for cancer-producing substances has not been demonstrated, and the benefits derived from the use of these color additives are not sufficient to justify running even a very small risk. The President's Science Advisory Committee, though, has come out for allowing some discretion in deciding whether a substance should be allowed, on the grounds that the Delaney clause could lead in some cases to costly restrictions without adding more than negligibly to the public protection.

New Policy on Grants to Colleges

Beginning this summer the National Science Foundation, the principal government source of support for basic research, will give colleges unrestricted grants amounting to 5 percent of their project grants during the past year.

The total amount of money involved, less than $3 million nationwide, is not large, but these "institutional grants" are viewed as an important step away from the policy of tying all grants to specific, preapproved projects.

New Society Directory Being Compiled by Academy

The National Academy of Sciences–National Research Council has announced that the seventh edition of *Scientific and Technical Societies of the United States and Canada* is now being compiled. The sixth edition was published in 1955.

Questionnaires have been mailed to approximately 2000 societies, and publication of the directory is scheduled for late 1960 or early 1961. This, like earlier editions, will represent a joint effort of the United States' National Academy of Sciences–National Research Council and the National Research Council of Canada.

Coverage in the directory will be limited to national and local professional membership organizations. Trade associations and institutions composed principally of paid staffs will not be included.

United States societies wishing to be included in the directory are asked to notify the Librarian of the National Academy of Sciences–National Research Council, 2101 Constitution Ave., NW, Washington 25, D.C. Canadian societies should contact Mr. Robert A. Lay, Public Relations Office, National Research Council, Ottawa 2, Ontario. The final acceptance date for information about a society is 15 July.

Foundation Surveys American Scientists

Last month about 250,000 scientists began reporting professional and economic data about themselves for the National Science Foundation's 1960 National Register of Scientific and Technical Personnel. Simultaneously, the foundation released data compiled from registration figures for 1956–58.

The median annual salary of U.S. scientists in 1956–58 was $7900. The 1958 National Register survey shows that scientists in chemical engineering and the medical sciences had the highest median salary (over $10,000), while those in the agricultural and biological sciences had the lowest, with median salaries below $7000.

About 137,000 full-time employed scientists reported in 1956–58. Almost half were in private industry or were self-employed, 28 percent were employed by educational institutions, and 14 percent were civilian employees of the federal government.

The median age of scientists reporting in 1958 was 38 years. About half were engaged in research and development activities and 16 percent in teaching.

Those in management or administration had the highest median salary ($11,000), those in teaching the lowest ($6500).

Eight national scientific societies are cooperating with the National Science Foundation in the registration of scientists—the American Chemical Society, American Geological Institute, American Institute of Biological Sciences, American Institute of Physics, American Mathematical Society, American Meteorological Society, American Psychological Association, and Federation of American Societies for Experimental Biology. It is hoped that scientists who do not receive questionnaires will contact the scientific society most appropriate to their specialties, so that they may be included in the National Register.

News Briefs

AMSOC Albatross Award. The third Albatross Award of the American Miscellaneous Society will be made in Helsinki, Finland, while the International Union of Geodesy and Geophysics is in session, 26 July to 6 August. The award consists of a stuffed, mature Laysan albatross (*Diomedea immutabilis*) with the names of the winners inscribed on the mounting base. It is given for the most unusual contribution to oceanography. The second Albatross Award was presented to Walter Munk at the International Oceanographic Congress held in New York in August 1959. The winner may keep the Albatross only until the next recipient is chosen. Announcement of the third award will be made in Helsinki.

* * *

Religion and science. The Institute on Religion in An Age of Science will hold its 7th annual conference at Star Island, N.H., 30 July–6 August. The theme of this year's conference is "Toward An Integration of Religion and Psychological Concepts on Man." Invited addresses will be given by David Bakan, Ralph Burhoe, L. K. Frank, O. H. Mowrer, Gene Nameche, Ira Progoff, and Joseph R. Royce. In addition, institute members A. G. Huntsman, Harlow Shapley, Huston Smith and others will participate as seminar and discussion leaders. Burhoe and Royce are serving as co-chairmen of this year's conference, with assistance from W. H. Clark. For information write to Ralph Burhoe, Executive Officer, American Academy of Arts and Sciences, 280 Newton St., Brookline 46, Mass.

* * *

Biophysics training. A recent survey of biophysics training programs in the United States and Canada showed that at least 51 institutions have such a program and that five others expect to initiate programs in the near future. Thirty-two of the institutions reporting offer advanced degrees in biophysics, 28 at the doctoral level. Thirteen medical schools now offer some instruction in biophysics to medical students. The survey was conducted by Norman A. Coulter, Jr., of Ohio State University.

* * *

Engineering at Purdue. Purdue University has announced the merger of the Division of Engineering Sciences and the School of Aeronautical Engineering to form a new school to be known as the School of Aeronautical and Engineering Sciences, effective 1 July. An Aero-Space Sciences Laboratory has been established for graduate research. Harold M. DeGroff will be head of the new school and director of the laboratory. E. A. Trabant, head of the Division of Engineering Sciences, had previously resigned to become dean of engineering at the University of Buffalo.

Scientists in the News

W. O. Reinhardt, professor of anatomy and head of the department at the University of California School of Medicine in San Francisco, is the first recipient of the Alan Gregg travel fellowship in medical education of the China Medical Board of New York, Inc. Reinhardt plans to spend 9 months in the Far East, accompanied by his family, beginning early next year. Most of his study will be conducted in the department of anatomy at Kyoto University Medical School in Japan. The Gregg fellowship was established last fall to honor the memory of Alan Gregg, a world-famous leader in medical education until his death in 1957.

The U.S. Department of Agriculture has presented Distinguished Service Awards to seven employes for outstanding achievements in research and

administration: **Henry L. Ahlgren**, director, Wisconsin Extension Service; **Edward C. Crafts**, assistant chief, Forest Service; **Gladys G. Gallup**, director, Extension Research and Training; **Herbert L. J. Haller**, assistant to the administrator, Agricultural Research Service; **Edward F. Knipling**, director, Entomology Research Division; **William D. Termohlen**, retired agricultural attaché to Japan; and **Harry C. Trelogan**, assistant administrator of the Agricultural Marketing Service.

R. Earl Storie will retire on 1 July after 39½ years of service with the University of California. He is professor of soils and plant nutrition and soil technologist in the California Agricultural Experiment Station. He is chairman of the California Soil Correlation Committee and consultant in the California Soil-Vegetation Survey. Storie is known as the originator of the Storie-Index for rating soils and of other land-classification techniques which are extensively used in this country and abroad.

General **Matthew B. Ridgway** has retired as chairman and chief executive officer of the Mellon Institute, Pittsburgh. **Paul Mellon** was elected chairman to succeed Ridgway; the latter was re-elected a trustee and will serve as a consultant to the institute.

R. B. Woodward, a leader in the laboratory synthesis of complex chemical substances, will become the Donner professor of science at Harvard University on 1 July. Woodward has carried on his research and teaching at Harvard since 1937, and since 1953 has been Morris Loeb professor of chemistry.

Murray M. Copeland has resigned as professor of oncology at Georgetown University Medical School (Washington, D.C.) to go to the University of Texas' M. D. Anderson Hospital and Tumor Institute at the Texas Medical Center, Houston, where he will become assistant director for education, effective 1 July.

Truman G. Blocker, Jr., will become chairman of the department of surgery of the University of Texas Medical Branch, Galveston, on 1 July, when he succeeds **Robert M. Moore**, who is resigning as chairman in order to reduce his administrative responsibilities.

Virgil Boekelheide, professor of chemistry at the University of Rochester, has been appointed professor of chemistry, University of Oregon, Eugene, effective 1 June.

Two members of the Faculty of Public Health at Harvard University, **Martha M. Eliot** and **Bertha S. Burke**, will retire on 1 July. Dr. Eliot will become professor emeritus of maternal and child health and Mrs. Burke will become professor emeritus of maternal and child nutrition. The two scientists are the third and fourth women to be so honored by the university.

Halsted R. Holman has been named Guggenhime professor and executive of the department of medicine at Stanford University School of Medicine, effective 1 July. Holman, the first appointee to the new professorship, is now on the staff of the Rockefeller Institute for Medical Research in New York.

He succeeds **David A. Rytand**, Bloomfield professor of medicine, as head of the department. Rytand has been departmental executive since 1954 but asked to be relieved of his administrative duties 2 years ago. He remains on the faculty as Bloomfield professor.

Saul Krugman, associate professor of pediatrics at New York University Medical Center, has been appointed professor and chairman of the department of pediatrics at the center, effective 1 July. He succeeds **L. Emmett Holt, Jr.**, who is retiring to devote full time to his research activities within the department.

Herbert E. Warden of Minneapolis, Minn., has been appointed associate professor of surgery at West Virginia University, effective 1 July. A heart surgeon and medical research scientist, Warden is the first appointee to the staff of **Bernard Zimmermann**, who recently was named professor of surgery.

Gerald Litwack, formerly associate professor of biochemistry at Rutgers University, has assumed the positions of director of biochemistry, Division of Cardiology, Philadelphia General Hospital, and research associate professor of biochemistry in medicine of the Graduate School of Medicine, University of Pennsylvania. He also has been named an honorary professor at Rutgers University.

Lawrence I. Miller has received the J. Shelton Horsley Research Award of the Virginia Academy of Science. Miller, who is professor of plant pathology at Virginia Polytechnic Institute, was honored for his paper on "The Influence of Soil Components on the Survival and Development of the Sting Nematode."

Harold V. Anderson, who has taught chemistry at Lehigh University for 42 years, will retire on 30 June.

James A. Luker, professor of chemical engineering at Syracuse University, has been appointed chairman of the chemical engineering department in the university's College of Engineering.

Harold P. Stephenson, associate professor of mechanical engineering at Duke University, has been appointed chairman of the department of physics, Pfeiffer College, Misenheimer, N.C. He will assume his new duties on 1 September.

Recent Deaths

Edgar B. Burchell, Larchmont, N.Y.; 88; retired bacteriologist and serologist at the New York Eye and Ear Hospital who for 30 years taught anatomy and bacteriology at the New York University School of Medicine; known for his study of the seventh (facial) nerve and for his work in the preparation of specimens, particularly those showing the structure of the ear; 19 May.

Merrill K. Lindsay, New Haven, Conn.; 75; retired professor of orthopedic surgery at Yale University; 24 May.

Hajime Masamune, Japan; 74; outstanding Japanese worker on mucopolysaccharides; professor of medical chemistry at the Medical School of Tohoku University from 1942 until his retirement in March 1959; professor of biochemistry at Hokkaido University, 1934–42; 19 Oct. 1959.

Dudley J. Morton, New York, N.Y.; 76; orthopedic surgeon and authority on the evolutionary development of the human foot and of erect posture and human locomotion; in 1924 was appointed associate professor of anatomy in the College of Physicians and Surgeons at Columbia University, where he served until 1944; charter member, secretary and treasurer of the Association of Physical Anthropologists; 22 May.

Book Reviews

A History of Greek Fire and Gunpowder. J. R. Partington. Heffer, Cambridge, England, 1960. xvi + 381 pp. Illus. + plates. 70s.

The history of Greek fire and gunpowder is one of the few subjects in the history of chemistry and technology in which many people are interested, yet it is particularly here that much misinformation has strangely persisted. Not so long ago, even such an outstanding historian and explosives expert as Tenney L. Davis published some analyses, carried out to the second decimal point, of gunpowder identified as "8th century, Marcus Graecus," and he remained quite undecided about Berthold Schwarz. Now that we have Partington's great work, misinformation should rapidly disappear and indecision should be left to those questions which still remain open.

Partington develops the story from the sources, and the difficulties in understanding these sources may explain the history of errors in the field. Some of the difficulties are in the translation of the old documents. Partington cites sections from them in the original languages, including Greek, Latin, French, German, and Spanish. For example, the Chinese word *huo p'ao* was often translated as "cannon," even in translating documents dating back to a time before the word acquired that meaning. M. Berthelot once translated a word that may have meant "impure potash from burnt barley straw," as "barley." Arabic *bundūq* gradually changed its meaning, in time, from "hazelnut" to "firearm." Translating old documents is difficult, and even Partington errs at one point. He discusses names of guns and mentions Dulle Griete in Ghent (1382) with the aside "(griete = great)" (page 128). Actually, however, *Griete* is *Grete* (Peggy or Maggy). A few pages before, Partington cites Christine of Pisa (1363–1431) as saying that guns were usually given female names, for example, *Garite* = Margaret! Naturally such a minor slip is here mentioned in the spirit in which Horace complained that sometimes Homer slept.

Together with the meaning of single words, the general attitude of the old authors has to be considered. Fire and explosion were terrifying experiences, easily exaggerated into fabulous tales. Chinese reports of 1345 describe the siege of a city in 1277 when the discharge of a cannon made the walls crumble and the 250 defenders disappear "without a trace," while the soldiers outside the city died of fright (page 244 ff). An accidental explosion under Akbar in 1367 hurled the bodies "miles away" (page 220).

In some instances, uncertainty about dates has to be added to all these difficulties. This is particularly true for the *Book of Fires of Markus the Greek* which Partington discusses in the second chapter, after "Incendiaries in warfare." This Markus is shown to be "a purely imaginary person" (page 40). The real author of the book was a Jew or a Spaniard, of the 12th or 13th century, who combined old and "new" information, that on gunpowder being the newest of all.

"The legend of Black Berthold" (chapter 3) had already been exposed by G. Köhler in 1887 and by M. Berthelot in 1891. Yet records of the 15th century treat him as a real person, a Dane or a Greek; an imaginative, 15th-century picture of him hangs in the Uffizi Museum in Florence, and a monument erected to him in Freiburg (Baden) claims him for Germany. Partington is rightly critical of those German military historians—for example, Berthold Rathgen (1847–1927)—who allowed national prejudice to influence their otherwise scholarly work.

"Now that the ghost of Berthold Schwarz had been laid and the record of 1313 for Ghent and, as will be seen presently, one of 1324 for Metz, removed as forgeries, we are left in suspense" (page 96). We are left with 1326 as the date for the earliest gun; this gun is shown in a manuscript by Walter de Milemete at Christ Church, Oxford, and is reproduced here as the frontispiece. A few guns were used by the English in the famous battle of Crecy (1346), but the noise was perhaps more effective than the shot (page 105). From then on, reliable reports about guns become more frequent, although other reports are quite doubtful. Chapters 4 and 5 abound in details on firearms and gunpowder. The military history of the Crusades and the sieges of Constantinople and Nikopolis are fully discussed. A fine description of gun casting from Kristoboulos, "1467 or later," is given (page 125 ff).

The long description of what has been reported and argued concerning pyrotechnics and firearms in China (chapter 6) concludes with first dates of about 900 for saltpeter, 1044 for a "proto-gunpowder," and true gunpowder only when it was also known in the West. That gunpowder was used "in different parts of the world" was stated by Roger Bacon in his *Opus Tertium*, 1266-68 (page 77).

One of the crucial points is to know when the words in the old texts really mean saltpeter. Partington discusses this in several places and devotes the final chapter to the discovery and manufacture of saltpeter. Since some of the ancient words interpreted as saltpeter also signify alkali (soda), an excursion on soap is included. "A satisfactory history of soap has still to be written" (page 309). The text ends with a compilation on compositions of gunpowder. The work of Berthollet and Berthelot is mentioned, but the several intermediate investigations—for example, by Proust shortly after Berthollet and by Bunsen before Berthelot—are missing.

The scholarly apparatus is impressive. Of the more than 1700 notes, some are quite extensive. The four indexes will help the reader to find his way through the complex story. Such a guide is necessary, because Partington has not always hewn a straight path through the jungle. Sometimes he retraces his steps or changes the focus on time. Yet, a careful reading of his book will be highly rewarding in many respects. All historians, not only those of chemistry and technology, will need it, and the general reader can find much to capture his interest. A lot of incidental information is presented, such as that on shrapnel—named after its inventor,

Lieutenant Henry Shrapnel, R.A., in 1784 (page 166)—or on the amazing predictions by Roger Bacon (page 72) and Giovanni da Fontana (page 160 ff). A particular joy are the general remarks with which the book is liberally sprinkled: on scholasticism (page 64) or on governments and history (page 187).

This rich and many-dimensional book is a highly valuable addition to Partington's outstanding historical work.

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Metallurgical Society Conferences. vol. 1, *Flat Rolled Products*. Rolling and treatment. T. E. Dancy and E. L. Robinson, Eds. xix + 128 pp. Illus. $3.75. vol. 2, *Reactive Metals*. W. R. Clough, Ed. xiv + 610 pp. Illus. $15. vol. 3, *Quality Requirements of Super-Duty Steels*. R. W. Lindsay, Ed. x + 309 pp. $8.50. vol. 4, *Physical Metallurgy of Stress Corrosion Fracture*. Thor N. Rhodin, Ed. xiii + 394 pp. Illus. $13. Interscience, New York, 1959.

Under the series title, *Metallurgical Society Conferences*, proceedings of technical conferences sponsored by the Metallurgial Society of the American Institute of Mining, Metallurgical and Petroleum Engineers are being published by Interscience Publishers. The first four volumes include: volume 1, *Flat Rolled Products*; volume 2, *Reactive Metals*; volume 3, *Quality Requirements of Super-Duty Steels*; and volume 4, *Physical Metallurgy of Stress Corrosion Fracture*. Additional volumes on recent and forthcoming conferences are scheduled for publication and the continuing series will cover the entire spectrum of metallurgy. Publication of the conference volumes serves to preserve and make widely available the scientific and technical information presented at the conferences.

The first volume contains four papers on rolling and annealing of steel, together with discussion of the papers and a panel discussion on the surface texture of flat rolled products; this is the proceedings of a conference-symposium held at Chicago, Ill., on 21 January 1959. The applied technical information presented at the conference and recorded in volume 1 is available in no other publication and will be of interest to anyone concerned with flat rolling of steel.

Volume 2 is the proceedings of the third Reactive Metals Conference, held at Buffalo, N.Y., 27–29 May 1958. Thirty-seven papers and the discussions that followed are presented. The topics cover a wide range of subjects on 11 reactive metals and the special processing techniques necessary for these unusual metals. Engineers concerned with nuclear reactor materials; moderately high-temperature, light-weight structures; and very high temperature structures will find the material in volume 2 useful in their work.

The proceedings of a technical conference on quality requirements of super-duty steels, held in Pittsburgh, Pa., 5–6 May 1958, are recorded in volume 3. Steels to meet special service requirements, such as very high strength, high temperature, or low temperature are considered. Four papers were presented in sessions on each of the following general topics: environment of use and required properties; relationship between composition structure and properties; air melting practices; special steelmaking practices. Volume 3 contains information helpful to both the producer-metallurgist and the consumer-metallurgist in understanding each other's quality requirements.

A symposium on the physical metallurgy of stress corrosion fracture was held at Pittsburgh, Pa., in April 1959; and the proceedings were published as volume 4 of this series. This volume contains 16 papers by well known workers in the stress corrosion field. It will be a welcome addition to the library of all those concerned with stress-corrosion.

O. CUTLER SHEPARD
Department of Metallurgical Engineering, Stanford University

Shots without Guns. The story of vaccination. Sarah R. Riedman. Rand McNally, Chicago, Ill., 1960. 232 pp. Illus. $3.50.

Man's war against his unseen enemies makes no less exciting reading than his battles against visible foes, especially when told by an author who combines insight into the scientific and human aspects of the fight against disease with a sense for the drama involved in the adventures of the giants who waged this fight. Whether the inquiring Jenner, the genial Pasteur, the meticulous Koch, the imaginative Ehrlich, or the cautious Salk is pictured, the portraits are always impressive and lively. While we are fascinated by the life history of the heroes who conquered ignorance and disease, we refresh our memory or learn new aspects of bacteriology, virology, and immunology.

FRANCIS JOSEPH WEISS
Arlington, Virginia

Anatomy of Seed Plants. Katherine Esau. Wiley, New York, 1960. viii + 376 pp. Illus. $6.95.

Katherine Esau is a plant anatomist of distinction, with a particular interest in the developmental anatomy of flowering plants. In 1953 her well-known *Plant Anatomy*, a book of 735 pages, was issued by John Wiley and Sons. The present briefer text is very welcome, for the older book, with all its many virtues, fell short, owing to sheer volume, of being an ideal text for a one-semester course. The present text is not a condensation of the older book but has been entirely rewritten, and fewer than 20 percent of the illustrations are from *Plant Anatomy*. The nature and sequence of the topics, after two short introductory chapters on the embryo and the development of the adult plant from the embryo, do not differ radically from those of the *Anatomy*: histology of tissues; primary and secondary growth in root and stem; the leaf, flower, fruit, and seed. The emphasis, wherever possible, is on development. The photomicrographs used as illustrations are scattered through the text instead of being concentrated at the end, as in the *Anatomy*. In general, the illustrations are of the high quality we have come to expect from Katherine Esau, but a few are too greatly reduced, and one plate of photomicrographs is poorly reproduced. The student who judges from the size of this relatively slender volume that his powers will not be taxed unduly will be badly deceived, for this is a tight, closely written book, into which the author has packed an astonishingly large mass of information. Among the innovations are a key to the microscopic identification of certain woods and a very extensive and useful glossary.

This is a book of high quality, scholarly and accurate. Few plant anatomists today could have prepared a volume of this general excellence, and Katherine Esau has earned the gratitude of her fellow teachers for providing them with an outstanding text in a field so basic and so necessary to other disciplines.

C. L. WILSON

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Beaches and Coasts. Cuchlaine A. M. King. Arnold, London; St Martin's Press, New York, 1959. xii + 403 pp. Illus. $14.50.

The intersection at the shoreline of land, sea, and air brings into dynamic interplay a wide variety of physical, chemical, and biological factors which produce an environment of remarkable diversity and great stress. Its characteristics as a joint boundary zone between the three major realms of the earth have given the shoreline an importance in terrestrial evolution and human affairs far out of proportion to its insignificant areal extent and have stirred the minds, the emotions, and the pens of men as few natural phenomena have.

It is principally in the last few decades, however, that interest has been focused on the mathematical description and experimental replication of the purely physical processes that shape and move the materials of which the shore is made. *Beaches and Coasts* is a valiant attempt to summarize this work, integrate it with field observations, and produce a modern synthesis of coastal processes and evolution. More than a third of the book is concerned with waves and wave action, but chapters are also devoted to the main variables affecting the beach, methods of study, wind, the movement of material, beach gradient and profiles, and the classification and development of coastal types. Documentation is drawn mainly from recent British and American publications, and from the North Sea and Channel coasts.

In view of the large volume of useful information which the author has assembled and condensed, it seems a pity that she did not take the additional steps necessary to achieve the logical cohesiveness and critical balance one has come to expect of British scientific books. The individual chapters in *Beaches and Coasts* are essentially independent essay reviews, each interesting and useful in its own right, but with very little interconnection. The disjointed effect is increased by a haphazard sequence of chapters, each having an independent list of references. Some subjects are repeatedly reintroduced (for example, the 1953 North Sea storm surge), while others that one might expect to find examined in such a book are left out or are only superficially treated (for example, biological processes, climatic variation, applications).

Taken separately, however, these chapters provide a useful introduction to the subjects considered. With few exceptions (for example, amphidromic systems), theoretical concepts and experimental data seem to be accurately grasped and are understandably presented. Much interesting observational information is also summarized, especially with regard to wave evolution, depth of wave action, and movement of sediments. The importance of onshore movement of sediments and coastal accretion is stressed and is backed up with some surprising statistics—the British Isles are reported to be gaining area from coastal accretion six times as fast as they are losing it by coastal erosion.

PRESTON E. CLOUD, JR.

U.S. Geological Survey,
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The Face of the Ancient Orient. A panorama of Near Eastern civilizations in pre-classical times. Sabatino Moscati. Quadrangle Books, Chicago, Ill., 1960 (published in Italy as *Il Profilo Dell'Oriente Mediterraneo*). xvi + 328 pp. Plates.

For well over a century now, archeologists have been excavating ancient cities and towns, temples and palaces, houses and tombs, all over the Near East from Egypt to India and from the Caspian Sea to the Persian Gulf. They have uncovered innumerable artifacts and art objects made of stone, metal, and wood, as well as myriad inscriptions on clay, stone, papyrus, and metal. This enormous treasure of archeological and epigraphic material has been made available primarily in the form of specialized and technical monographs, reports, and articles which have accumulated over the decades to ominous proportions. Only rarely have more general studies and evaluations of this conglomerate mass of archeological sources been attempted, and these usually have taken the form of political and cultural histories of one or another of the rediscovered and resurrected peoples of the Ancient Near East. More recently, however, a number of scholars and humanists, taking their cue from the physical sciences and their current high prestige, have begun thinking and writing of "organic wholes" and universal laws in history and culture, even utilizing at times the language and terminology of the physical sciences. The book under review reflects this recent, and in some ways not unpromising, approach as it is being applied to the history and culture of the Ancient Orient.

The physical science that seems to have a special attraction for Moscati is chemistry, and so we find the book divided into sections labeled "The components," "The catalysts," and "The synthesis." The reader should not be discouraged or misled by this rather superficial chemical "facade"; actually Moscati organizes the relevant historical, literary, and cultural data with considerable skill, and presents it in "orthodox" scholarly language with all its virtues and defects. After an introductory chapter outlining the area and the time to be covered, the book proceeds to delineate briefly and eclectically the history, religion, literature, and art of all the more important peoples of the Ancient Orient: Sumerians, Babylonians and Assyrians, Egyptians, Hittites and Hurrians, Canaanites and Aramaeans, Israel, and Persians. The closing chapter, to be sure, introduces once again scraps of "scientific" terminology, such as *isoid, reagents, component elements, catalyst,* and *compound.* But the title of the chapter, "The face of the Ancient Orient" sounds more like alchemy than chemistry, and the conclusions it presents are no more precise, accurate, or valid than those found in historical works that "stick" to the customary humanistic diction with its "built-in" vagaries and shortcomings.

Psychologically speaking, there are two contrasting types of scholars: the specialist who digs an inch wide and a mile deep, and the generalizer who digs a mile wide and an inch deep; both are indispensable to creative scholarship, and the two are equally pleasing in the eyes of Jahweh. Moscati combines to some extent the virtues of both types, but his heart, head, and hand are with

the second. In preparing his broad panoramic book, *The Face of the Ancient Orient*, he necessarily had to resort to secondary sources for most of his data; so a rather narrow Sumerologist like myself, when reading his chapter on the Sumerians, cannot help being pained and depressed by some of his oversimplifications, misunderstandings, and quotations from outdated and untrustworthy translations. By and large, however, this book presents a lucid, intelligent, and lively summation of some of the more important aspects of the culture of the Ancient Orient, and I recommend it warmly for use in schools and colleges, and for the culturally minded layman, as an appetizing and stimulating introduction to the study of man's early civilizations.

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Sinopsis de la flora chilena, claves para la identificacion de familias y generos. Carlos Muñoz Pizarro. Ediciones de la Universidad de Chile, Santiago, 1959. 840 pp. Illus. Paper, $14.

The flora of Chile is a key piece, and usually a missing one, in attempted comparisons of the plant life of the temperate Americas or in any analysis of the vegetation of the isolated portions of the southern temperate zone. It may come as a surprise to those who blissfully assume that the pioneering labors of plant taxonomy were happily consummated in the earlier 19th century and digested by Darwin, that there has never been anything like a complete manual to serve as an introduction to this taxonomically and phytogeographically rich and interesting flora. The classical floristic account of the country by Claudio Gay is now more than a century old, and the more recent (1896–1911) studies by Carlos Reiche did not cover vascular cryptogams, gymnosperms, or "amentiferous" dicotyledons. In addition to this lamentable situation succinctly pointed out by Carl Skottsberg in his prologue to the present volume, there is the unmentioned fact that the uncritical multiplication of taxa by the great naturalist, R. A. Philippi, in his later years, has effectively frustrated work on the Chilean flora.

Muñoz, well known in this country from his graduate days at Harvard and his subsequent participation in various conferences and congresses, does not offer his *Sinopsis* as the full answer to the acknowledged lack. Indeed, his present work is visualized as a necessary and useful preliminary to a truly modern flora of Chile. His work is heavily indebted to the assistance of Benkt Sparre, and he has had the aid of most of the Chilean botanists and that of all of us who were fortunate enough to be able to visit Chile and to enjoy the unfailing hospitality of the author and his compatriots during the more than a decade of the book's gestation.

As indicated by the subtitle, the *Sinopsis* consists essentially of 220 pages of carefully executed dichotomous keys to some 91 orders, 182 families, and the nearly 1000 genera of vascular plants believed to be native to, or established in, mainland Chile and its insular possessions. The latter include the fabulous archipelago of Juan Fernández, so beautifully monographed by Skottsberg, the offshore islets of San Félix and San Ambrosio, and the strongly discordant Isla de Pascua (Easter Island). Each family is provided with a brief diagnosis, a list of some of its more common and better known species, and a key to its genera. There are no generic descriptions, but their lack is more than compensated for by the provision of more than 200 full-page analytical plates prepared by the skilled botanical illustrators Fusa Sudzuki de Meza and Eugenio Sierra R. This central core is buttressed by a 25-page glossary, largely adapted from the excellent botanical dictionary of Font Quer; a 26-page bibliography lists the works consulted in the preparation of keys and descriptions.

A useful tabulation of the authors of taxa described from Chile provides a digest of the country's botanical history. It is notable that only four of those listed as contemporaneous are Chileans: Acevedo de Vargas, Espinosa, Kausel, Looser. This is not an entirely fair picture of present-day Chilean plant taxonomy, although economic conditions and professional opportunities in recent decades have not been the sort that encourage botanical careers. There is some heartening evidence that the scientific climate may be improving. Active groups of plant scientists have developed in Concepción, Santiago, and perhaps elsewhere. However, a great deal of basic exploratory effort and simple amassing of material still remains to be done, while senseless destruction of the beautiful and unique vegetation proceeds apace. To my knowledge, there is nothing like a "complete" reference collection of the Chilean flora anywhere in the world; to attempt work with any part of it necessitates a large expenditure of time and effort in locating and accumulating even the basic materials.

The appearance of Muñoz's useful and very attractive volume suggests that there is now some official backing for a thorough appraisal of the country's natural resources. The *Sinopsis*, excellent as far as it goes, may also be a symbol that basic scientific investigation in Chile can anticipate public understanding and support. The realization of a modern Chilean flora may thus be less of a mirage than it has long appeared to those awaiting it.

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Miscellaneous Publications

(Inquiries concerning these publications should be addressed not to Science, but to the publisher or agency sponsoring the publication.)

The Future of Latin American Exports to the United States: 1965 and 1970. Louis O. Delwart. And a statement by the Interamerican Research Committee. National Planning Association, Washington, D.C., 1960. 127 pp. $2. The Interamerican Research Committee, established by the National Planning Association, has approximately equal representation from the peoples of the Western Hemisphere. In this report, the first of a series, the committee warns that "The export situation of Latin America . . . requires urgent and sympathetic attention" and that in the past few years a "virtual stagnation in the overall volume of Latin American exports to the United States" has already contributed to a slow-down in the rate of growth of the Latin American economy as a whole. The report warns that "the inability to find needed markets in the United States may induce these countries to accept bilateral agreements with the countries of the Soviet Bloc."

Japanese Journals in Science and Technology. An annotated checklist. Compiled by George S. Bonn. New York Public Library, New York, 1960. 134 pp. $2. A list of 660 titles selected from approximately 900 journals examined by Bonn. Arrangement is by subject, with each subject subdivided into six major types of publishing bodies—societies, government agencies, industries, private publishers, pre-1948 universities and post-1948 universities.

Laboratory Exercises in Invertebrate Physiology. John H. Welsh and Ralph I. Smith. Burgess, Minneapolis, rev. ed., 1960. 179 pp. $3.50.

Science and Engineering in American Industry. Report on a 1956 survey. National Science Foundation, Washington 25, 1960 (order from Supt. of Documents, GPO, Washington 25). 117 pp. $0.70.

Reports

Effects of Absence of Saliva on Blood Feeding by Mosquitoes

Abstract. The salivary glands of *Aedes stimulans* (Walker) are the source of an antigen which produces typical bite reactions in men and laboratory rabbits. If the main salivary duct is cut, the reaction is not produced when the mosquito bites. Lack of saliva does not affect the intake or movement of blood into the mid-gut, nor does it prevent the development of eggs. The presence of an anesthetic component in saliva is suggested.

The salivary secretions of a parasitic animal have two important aspects: the effects of introduction of the saliva into the host tissues, and the influence of salivary secretions upon the feeding mechanism and subsequent digestive processes of the parasite.

When a mosquito feeds upon a mammalian host a wheal commonly develops around the puncture site; in man wheal formation is usually accompanied by erythema and irritation. Wheals may also be produced in laboratory rabbits in response to a series of bites received over a period of time (*1*). Controlled experiments with rabbits have established the fact that the reaction to mosquito bites is of a hypersensitive type, and therefore occurs in response to the injection of antigen (*1*).

It has been generally assumed that the source of this antigenic material lies in the salivary glands, but no direct evidence for this assumption has been recorded. The following technique was devised to determine if a bite, unaccompanied by salivary secretion, would produce the characteristic symptoms in a sensitized human host.

Aedes stimulans (Walker) occurs abundantly in eastern Ontario during the summer months, and this comparatively large species was selected for initial attempts to sever the main salivary duct leading to the hypopharynx. Females were captured as they attempted to feed on collectors in the field and also were reared from field-collected larvae. All adults were maintained on 0.1*M* sucrose until they were required.

To cut the salivary duct, the female was first anesthetized with carbon dioxide and placed on its back on a slide. Strips of plasticine were crossed over the abdomen to hold the insect in place and the proboscis was gently strapped out in a position which extended the neck.

A needle of tungsten wire (diameter 10 μ), sharpened to a fine point, was used to make a small incision into the anterior region of the neck, exposing the dark salivary duct. Posteriorly this duct divides into two branches which lead to the tri-lobed glands on each side (Fig. 1). The duct was cut in the anterior position with a sharpened sliver of tungsten wire. The operated mosquito was transferred to a vial and supplied with sucrose solution. Mortality rates were high, undoubtedly because of handling techniques and also the variation in age of mosquitoes caught in the field.

After a recovery period of 24 hours the operated mosquito and an unoperated control were offered a blood meal from the arm of a human subject. Many of the surviving operated mosquitoes did not attempt to bite; however, 12 individuals became fully engorged and gave clear results. The bite of a female whose salivary duct had been cut failed, in every case, to produce a wheal or irritation; the bites of control mosquitoes, in an adjacent area on the arm, produced the normal reaction. These experiments give very good evidence that the source of antigenic material is the salivary glands.

The technique described offers an opportunity for study of the effect of salivary secretions upon digestion and utilization of the blood meal, and also of the possible role of saliva as a lubricant during insertion of the mouthparts.

A robust survivor with the salivary duct cut appears to experience no difficulty in inserting the mouthparts into the skin of the host. This is not surprising since the salivary canal in the hypopharynx is enclosed throughout its length, and saliva does not have access to the mandibles and maxillae except at the tip of the proboscis. Movement of the blood from the mouthparts to the mid-gut appeared to be normal. An interesting comparison may be made with the feeding of *Glossina morsitans* Westw. and *G. tachinoides* Westw. after removal of the salivary glands (*2*). These operated tsetse flies fed readily but the ingested blood subsequently clotted in varying degrees in the proboscis, proventriculus, and crop. This, it was claimed, resulted from the absence of an anticoagulin normally secreted by the salivary glands.

Clotting of human blood has been shown to be prevented by addition of extracts of salivary glands of several species of *Anopheles*. Clotting time was not prolonged by extracts of glands of *Aedes aegypti* L., and other species of *Aedes, Culex,* and *Psorophora* (*3*). The effect of glandular extracts of *A. stimulans* on blood in vitro is not known.

Larsen and Bodenstein (*4*) have shown that the blood meal initiates a series of humoral activities in the brain and corpora allata of *C. pipiens* L. and

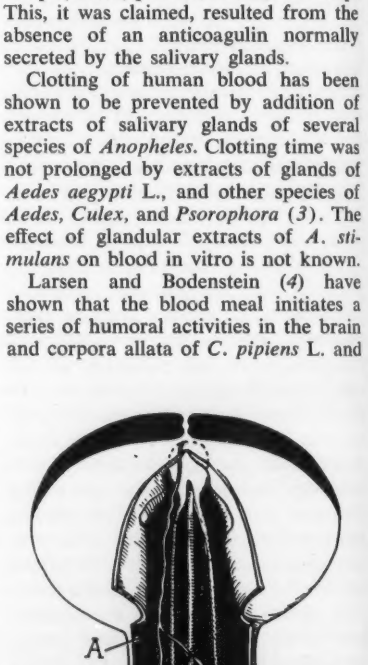

Fig. 1. Ventral view of the head, neck, and prothorax of a female *Aedes aegypti* mosquito dissected to show positions of the salivary ducts and glands. *A*, Branching of main salivary duct; *B*, proventriculus; *C*, left salivary glands.

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

A. aegypti. This work has provided good evidence that it is the distension of the gut by the clotted blood which triggers a humoral chain of events culminating in maturation of the eggs.

We were interested in discovering if the absence of saliva affected the subsequent clotting of the blood in the mid-gut and if this would in turn affect egg development. The mid-guts of operated and blood-fed *A. stimulans* contain a blood clot which does not appear to be different, on examination under the microscope, from the clot formed in a normal mosquito. Moreover, the eggs develop to stages F and G (*4*), indicating that humoral activity has been initiated.

Recently we have been able to cut the salivary duct in *A. aegypti*, and experiments with mosquitoes of known age and diet should offer more accurate data. The results of the experiments are at present difficult to interpret in terms of the function of the salivary glands of mosquitoes. If saliva plays a part in digestion of the blood meal, this function may be reflected in the subsequent activities of the mosquito, rather than in the development of the current batch of eggs. It has been suggested (*5*) that the salivary glands of blood-sucking arthropods are now nonfunctional but remain as evidence of a plant-feeding ancestry. This seems unlikely in mosquitoes, in view of an apparent cyclical activity of the gland cells (*6*).

The presence of an anesthetic component is suggested by observations that the bites of *A. stimulans*, unaccompanied by saliva, are more painful than the bites of normal mosquitoes. Such a component would have considerable adaptive significance and might also be a factor determining the effectiveness of certain species as vectors of disease organisms. The success of insects as vectors has been viewed in this way by Herms (*7*; *8*).

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2 March 1960

Residence Time of Dissolved Phosphate in Natural Waters

Abstract. Residence time varies from approximately 0.05 to 200 hours. Short residence times are indicative of depleted phosphate, active metabolic activity, or both. The turnover rate of phosphate is between 0.1 and 1.0 mg of phosphorus per cubic meter, per hour, regardless of phosphate concentration, except in biologically active systems where it is 1.0 to 20. The turnover rate of phosphate may be more important than the phosphate concentration in maintaining highly productive systems.

The use of $P^{32}O_4$ in studies of lakes has revealed dynamic equilibria between phosphate dissolved in the water and phosphorus in the plankton, benthic organisms, bacteria, sediments, and dissolved organic materials (*1–5*). The residence time of dissolved phosphate proved to be a matter of minutes (*3*). Estimates of the residence time of dissolved phosphate in the sea and in estuaries (Table 1) reveal a wide range of conditions under which phosphate equilibria may be established, with the residence time varying over several orders of magnitude.

Residence time and turnover rate of phosphate in freshly collected water were estimated in the laboratory. Aliquots of 1.5 liters of water were placed in 7-liter rolling bottles in a constant-temperature room, and the water was kept within ±2°C of its temperature at collection. The water was illuminated by flourescent lights with an intensity of 350 ft-ca. Sterile, carrier-free $P^{32}O_4$ was added to the water, and changes in radioactivity were measured at intervals until equilibrium was reached. Uptake of P^{32} by bacteria and plankton was measured by filtering aliquots of water through Millipore filters of 0.45-μ porosity and counting the activity of the dried filters. The residence time of phosphate was calculated by the method used in studies of lakes (*2–4*), but only the observations of Rigler (*3*) are comparable in the method of filtration.

Turnover rate is considerably less variable than residence time, and exceeds the range of 0.1 to 1.0 mg/m³ per hour only in biologically active systems, such as plankton blooms, salt marshes, and small lakes. Residence time is influenced both by the turnover rate and the concentration of dissolved phosphate. Therefore, a system having a short phosphate residence time may be impoverished in phosphate, as in the sea, or it may be unusually active biologically, as in algal blooms. When both conditions occur together, as in small lakes, the residence time becomes vanishingly short.

Measurements of the concentration of dissolved phosphate in natural waters give a very limited indication of phosphate availability. Much or virtually all the phosphorus in the system may be inside living organisms at any given time, yet it may be overturning every hour with the result that there will be a constant supply of phosphate for organisms able to concentrate it from a very dilute solution. Such systems may remain stable biologically and chemically for considerable periods in the apparent absence of available phos-

Table 1. Residence time, concentration, and turnover rate of dissolved phosphate in natural waters

Date	Location	Type of system	Dissolved phosphate			T (°C)
			Res. time (hr)	Concn. (mg atom P/m³)	Turnover (mg P/m³ per hr)	
9/28/54	Oiseau Lake, Ontario*	Small lake	0.06	0.003	1.6	
9/11 and 9/18/53	Toussaint Lake, Ontario*	Small bog lake	0.08	0.009	3.6	
9/17/54	Maskinonge Lake, Ontario*	Lake	0.4	0.012	1.0	
7/29/58	Salt-marsh creek, Georgia	*Kryptoperidinium* bloom	1.0	0.6	19	29
5/14/59	Altamaha River, Georgia	Nostocaceae bloom	1.0	0.1	3	25
7/18/58	30°53′N, 80°28′W	Continental shelf water	5	0.1	0.6	29
7/18/58	30°58′N, 80°01′W	Gulf Stream, surface	4	0.1	0.8	29
7/18/58	30°58′N, 80°01′W	Gulf Stream, 60 m	12	0.1	0.3	29
4/2/59	31°25′N, 81°05′W	Coastal sea water, surface	34	0.1	0.1	18
10/15/59	31°19′N, 81°10′W	Coastal sea water, surface	155	0.5	0.1	34
10/15/59	31°20′N, 81°13′W	Coastal sea water, surface	63	0.8	0.4	33
11/19/59	31°20′N, 81°13′W	Coastal sea water, surface	50	0.3	0.2	15
11/19/59	31°19′N, 81°11′W	Coastal sea water, surface	46	0.1	0.1	16
4/20/59	31°23′N, 81°17′W	Doboy Inlet	4	0.2	1.5	22
11/12/59	31°23′N, 81°17′W	Doboy Inlet	66	1.0	0.5	16
11/19/59	31°23′N, 81°17′W	Doboy Inlet	111	0.9	0.2	15
6/26/58	31°25′N, 81°18′W	Doboy Sound	37			
2/28/59	31°25′N, 81°18′W	Doboy Sound	50	0.6	0.5	12
7/17/59	31°25′N, 81°18′W	Doboy Sound	56	1.0	0.5	30
11/12/59	31°25′N, 81°18′W	Doboy Sound	39	1.0	0.8	15
11/19/59	31°25′N, 81°18′W	Doboy Sound	30	1.0	1.0	14
1/30/59	31°29′N, 81°16′W	Salt marsh (low tide)	49	3.0	2.0	15
10/12/59	31°29′N, 81°16′W	Salt marsh (low tide)	40	5.5	4.0	27
11/12/59	31°29′N, 81°16′W	Salt marsh (high tide)	169	1.1	0.2	15
11/12/59	Altamaha River, Georgia		13	0.2	0.5	14
9/14/54	Ottawa River, Ontario*		30	0.05	0.2	

* From Rigler (*3*). Rigler's observations have been converted from other units of measurement for convenient comparison.

phate. This suggests how it is possible for phytoplankton blooms to persist in water containing only a few hours' supply of dissolved phosphate. The observations presented here suggest that a rapid flux of phosphate is typical of highly productive systems, such as blooms, and that the flux rate is more important than the concentration of dissolved phosphate in maintaining high rates of organic production.

It would be of interest to learn what factors tend to stabilize the flux of phosphate over a wide range of phosphate concentrations and what factors induce a more rapid flux in certain circumstances (*6*).

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25 January 1960

Biological Effects of a Chemical Mutagen, Diepoxybutane, on Tomato

Abstract. Tomato seeds were treated with three concentrations of 1:2, 3:4-diepoxybutane. Seedling mutations were induced in high amounts with all doses, but other biological effects in the treated generation including delayed germination, chlorophyll-deficiency sectoring in the first true leaves, and failure of plants to bear fruit, were found only with the highest concentration. Neither a lethal nor a partially lethal dose was applied.

Diepoxybutane has been demonstrated to be a highly effective mutagenic agent in barley by Ehrenberg and Gustafsson (*1*), in *Neurospora* by Kolmark and Westergaard (*2*), and in *Drosophila* by Bird (*3*) and others. Other biological effects, occurring simultaneously with the mutational effect of diepoxybutane in these biological materials, included sterility and lethality, which increased with increased chemical dose. However, in this study lethality was absent in all treatments, and sterility was restricted to only the highest concentration, while the mutational effect was observed with all doses of diepoxybutane applied.

Tomato seeds, 300 per treatment, of the highly inbred O18 strain of the Red Cherry variety of *Lycopersicon esculentum*, were treated with three concentrations of 1:2, 3:4-diepoxybutane—0.2, 0.5, and 0.8 percent—in aqueous solution and with distilled water. Preliminary investigations indicated the 50 percent lethal dose to be somewhere between 0.1 and 1.0 percent diepoxybutane when the seeds, presoaked for 24 hours in distilled water, were treated in the chemical for 1 hour under vacuum followed by 5 hours at normal pressure. The same conditions of treatment were followed throughout.

Table 1. Number of mutations recovered in the second generation following treatment of tomato seed with various concentrations of diepoxybutane, or (control group) with distilled water.

Treatment	No. of transplanted seedlings	No. of mutants recovered	Percent of plants producing mutants
Control (distilled H_2O)	69	0	0.0
0.2 percent diepoxybutane	110	5	4.5
0.5 percent diepoxybutane	104	4	3.8
0.8 percent diepoxybutane	40	7	17.5

Following treatment, the seeds were thoroughly washed and planted in soil. After 2 weeks the seedlings were transplanted to pots on a raised bench in a greenhouse maintained at 65°F. night temperature. The number of transplanted seedlings in each treatment were as shown in Table 1. The plants, trained to one main axis, were grown for 7 months and seeds from the first four fruit-bearing inflorescences were recovered separately.

For the determination of induced mutations the seeds from the first and last inflorescence of each plant were grown to the seedling stage of the second generation and screened for seedling abnormalities. Lines segregating for color, rate of growth, or morphological seedling abnormalities were grown to the third generation to determine the inheritance of the abnormalities recovered. Abnormalities segregating in a 3:1 ratio in the seedling stage of the third generation were classified as mutations.

Mutations were recovered from all three diepoxybutane treatments but none from the control lots. The number and percentage of mutations recovered from each diepoxybutane treatment are listed in Table 1. Only a single mutant type was recovered from any one treated plant, but the mutation could be found in either or both of the tested inflorescences of this plant. The majority of the mutants were chlorophyll-deficient types; in the remainder, rate of growth and morphological characteristics were affected.

Apparent pleiotropic effects on rate of growth and morphological development were characteristic of many of the chlorophyll mutants. Lethality in the seedling stage or sterility of most of the mutants prevented the recovery of the mutations in a homozygous condition. This fact might be an indication of chromosomal deletions or rearrangements in the mutants rather than true gene changes.

Biological effects in the treated generation, excluding the mutational effect, resulting from diepoxybutane treatment were found only with the highest concentration, 0.8 percent diepoxybutane. The seed treated with 0.2 and 0.5 percent diepoxybutane developed in all respects the same as the control. The mutational effect in seed treated with these two concentrations was the only observable difference from the seed treated with distilled water.

The three characteristic effects of the 0.8 percent dieoxybutane treatment in the treated generation included delayed germination, chlorophyll-deficiency sectoring in the first true leaves, and the production of fruitless plants.

While the control, 0.2 percent, and 0.5 percent diepoxybutane treatments yielded 90 percent germination of the treated seed within 10 days of planting, during the same period only 15 seeds, or 5 percent, in the 0.8 percent diepoxybutane treatment germinated. However, 3 weeks later germination had increased to 90 percent, thus demonstrating the failure to administer even a partially lethal dose.

All of the seedlings that germinated in the 0.8 percent diepoxybutane treatment exhibited chlorophyll-deficiency sectoring in the first true leaves. All other leaves developed normally.

In this experiment, sterility was determined by the number of plants failing to produce any fruit; a treated plant was considered fertile if it produced at least one fruit on any of the tested inflorescences. In the 0.8 percent diepoxybutane treatment, eight plants, or 20 percent, failed to produce fruit. In the other two chemical treatments together, only one plant, of the 0.2 percent diepoxybutane treatment, was fruitless. However, all of the plants which failed to produce fruit in the greenhouse on the main axis, when they were pruned back to the first leaves and set in the field, produced fruit on their axillary branches in all respects the same as normal plants.

The tomato appears in this experiment to respond differently from other biological materials to diepoxybutane

treatment. While the mutational effect increased with increased chemical concentration, at least between the two higher concentrations, neither sterility nor lethality patterns were the same. Sterility was manifested only at the highest concentration of diepoxybutane, while even a partially lethal dose was not demonstrated with any of the concentrations employed. At the same time, two previously unreported effects, chlorophyll-deficiency sectoring and delayed germination, were produced by diepoxybutane treatment.

All of these effects, excluding the mutational effect, were restricted in tomato to the 0.8 percent diepoxybutane treatment. In addition, chlorophyll-deficiency sectoring in first true leaves and delayed germination were relatively constant features of this treatment.

The constancy of delayed germination and chlorophyll sectoring in the 0.8 percent diepoxybutane treatment indicates possibly that there is a relatively invariable response of the individual tomato seeds in a treatment to a given concentration of chemical. And, too, if one assumes these effects to be the result of gross chromosomal aberrations, the ratio of normal to damaged meristematic cells in the seed treated with 0.8 percent diepoxybutane has reached the threshold point necessary for the manifestation of these effects. On the other hand, if lethality is dependent on all meristematic cells of a seed being irreparably damaged, then the dosage necessary for this condition had not yet been administered in this experiment.

The results of this study may thus indicate the tomato to be a valuable material on which to study the quantitative effects of chemical mutagens in higher plants. First, diepoxybutane has demonstrated mutagenic effects over a wide range of chemical concentrations (0.2 to 0.8 percent) without being limited by lethality or sterility. Second, the seed itself demonstrated few properties that would cause variations in the effects of a given concentration of chemical with an individual treatment (*4*).

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26 February 1960

Effect of Synthetic Smog on Spontaneous Activity of Mice

Abstract. Mice allowed to run in a revolving-drum activity cage are sensitive indicators of air pollution. They respond to the presence of synthetic smog by diminishing their 24-hour activity in the revolving wheels. The reduction in wheel activity is comparatively greater for larger amounts of smog.

Since Stewart's pioneering experiments on the measurement of the activity of rats and mice with revolving wheels and kymographs, numerous investigators have studied the factors which influence the behavior of rats in this type of apparatus (*1*). Attempts have been made to calibrate or improve the wheels, or invent new means of measurement, such as tambour-mounted or tilting cages, photoelectric or magnetic devices, and a variety of mazes, most of which measure different quantities (*2*). Furthermore, many environmental and biological factors affect activity, yet relatively little is known about the motivation involved, despite several investigations of this phase of the problem (*3*).

Some of the factors which tend to complicate the use of these techniques are age, sex, diurnal cycle, oestrus cycle, visible light (*5*), heredity (*6*), and hunger and dietary deficiencies (*7*). Drugs with both stimulating and depressing effects on activity are known (*8*). Whole-body radiation also exerts some influence, though activity is not especially responsive to this kind of insult (*9*). Tobacco smoke is also claimed to have some effects (*10*). Although other air pollutants have not been systematically studied, one of Stewart's original observations is of interest in this connection. During the course of his experiments on the effects of barometric pressure and alcohol on the activity of rats, he observed a decrease on several occasions which he attributed to the escape of gas in his laboratory. While he does not indicate whether the poisoning seriously affected his animals in other ways, he was apparently the first to observe the effect of an air pollutant on voluntary activity (*1*).

The experiments described in this report were performed in the expectation that biological methods which measure the voluntary behavior of the experimental animal would provide sensitive indicators of environmental factors such as air pollution and infectious agents, since relatively small sensory impulses may be amplified by the neuromuscular system of the animal into large changes in behavior (*4*).

For the purpose of studying these effects, we have employed two modified 100 ft³ refrigerators. The chambers are similar and are provided with activated charcoal filtered air pulled by an exhaust blower on the roof. The temperature is approximately the same in both chambers. A mixture of ozone and gasoline vapor in air is forced into the exposure chamber. (The technique is similar to that described by Kotin and Falk, *11*.) This smog is analyzed daily for total oxidant with phenolphthalein (*12*) and for ozone by absorption in neutral potassium iodide (*13*).

The mice used for the present study were young adult C57BL/6 males; they were caged individually, and they had

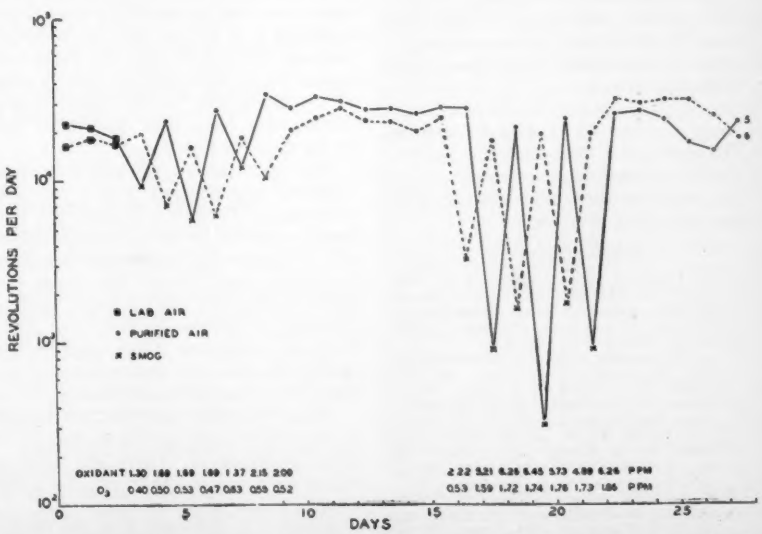

Fig. 1. Spontaneous wheel turning activity of two C57 Black male mice in different environments. The total oxidant and ozone determinations are shown at the bottom of the graph for each day of exposure to synthetic air pollutant mixture.

free access to laboratory chow. The rotating wheels, 6¾ inches in diameter, were lined with a plastic film in which fine carborundum powder is embedded for traction. Rotation is recorded remotely by electric counters activated by microswitches.

After approximately 3 weeks in the wheel cages, the mice stabilized their activity. The cages were then placed in the chambers, one in smog, the other in purified air. At intervals of 24 hours, the cages were exchanged between chambers, the smogged mouse being placed in filtered air, the filtered air mouse in smog. This process was repeated for a total of 6 days in light smog and 6 days in heavy smog; there was a 1-week interval in the filtered air chamber between the two periods of exposure to smog.

Figure 1 is a semilogarithmic plot of the daily activity records of two individual mice throughout one experiment. The smog concentrations in parts per million (ppm) for each exposure day are shown at the bottom. The regular manner in which low concentrations of smog diminish the wheel-turning is obvious and significant ($P = <.001$), by analysis of variance, as is the greater inhibition which occurred after the smog concentration was increased. The ozone concentration in the first series of exposures corresponds to a first-stage alert in Los Angeles (0.5 ppm), although the total oxidant values are somewhat higher. These experiments are easily repeatable with different kinds of wheels. Thus far, we have shown reduced activity in smog with a total of 14 mice. Furthermore, a decrease in activity is noted for at least 3 weeks when the mice remain in the smog chamber. The activity techniques, though little used for the study of disease, may be sensitive indicators of subclinical disturbances (*14*).

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Preservation of Honey Bee Semen

Abstract. Fertilized eggs have been obtained from queen honey bees (*Apis mellifera* L.) inseminated with sperm that had been stored in vitro at above-freezing temperatures for up to 68 days. The effects of various experimental storage treatments on semen are described. Semen shipped by ordinary mail has been successfully used for artificial insemination.

In some species of insects the semen is stored in the spermatheca of the female after mating. Sperms remain alive in the spermatheca from a few days in certain flies to a few years in some ants and in the honey bee (*Apis mellifera* L.). Queen ants are reported to have laid fertile eggs 15 years and queen honey bees 7 years after mating (*1*). Artificially inseminated queen honey bees have been known to lay fertile eggs for 3 years.

The spermatheca of queen bees is spherical and covered with a network of tracheae. Dissection of queens a few hours after death shows disintegration of all digestive and reproductive organs except the spermatheca. This organ appears fresh, and the enclosed sperms continue to be motile and have been used to inseminate other queens, an indication that the organ has a relatively impermeable membrane.

Attempts at low temperature storage of bee semen at Baton Rouge, La., have been unsuccessful. Therefore, experiments were undertaken to develop some other method of preservation.

Semen was collected from the ejaculate of 5 to 25 drones (*2*) and placed in capillary tubes 1.8 to 2.0 mm in diameter. Pooled samples that had been thoroughly mixed would have been desirable to eliminate differences in the drones' fertilization capacity, but the mixing of sperms in pooled ejaculates is not possible with present techniques (*3*).

Queens were inseminated by the method described by Mackensen and Roberts (*2*). Virgin queens were anesthetized, and semen was placed in their oviducts with a syringe. With this method, several million sperms usually reach the spermatheca and less than 5 percent of the queens fail to survive insemination. The inseminated queens were kept in small colonies of only a few thousand bees, so that the rate of egg laying was no more than 300 to 400 per day.

The first experiment was designed to determine the effect of the following environmental conditions on the viability of sperm stored from 7 to 33 days; (i) dilution with different media, (ii) replacement of the air atmosphere with various gases, and (iii) temperature. The diluent materials included a Ringer-buffer mixture, Ringer-buffer-fructose mixture (*4*), bee blood, and royal jelly. The volume of diluent was not more than the total volume of semen. After the diluent and semen were mixed in some of the tubes, the air above was replaced with carbon dioxide, nitrogen, or helium, by injection from a finely drawn glass tube; the tubes of semen were sealed by heating immediately after removal of the gas jet.

Of 105 queens inseminated, 31 produced fertile eggs and 17 others had sperm in the spermatheca but either did not lay fertile eggs or laid so few that their numbers were considered unreliable. Table 1 shows the storage treatments of sperm used with 14 queens that produced fertile workers. All queens that received semen treated with carbon dioxide died. Semen diluted with royal jelly or bee blood coagulated and could not be transferred to the inseminating syringe. Many of the tubes diluted with Ringer-buffer and Ringer-buffer-fructose had partially coagulated semen. Some tubes stored for 2 weeks or longer showed contaminating microorganisms, and the semen in them was

Table 1. Method of storage of honey bee sperm at defined temperatures.

Days in storage	Temp. (°F)	Gas in storage tube	No. of queens fertilized
Semen in Ringer-buffer solution adjusted to pH 7.4			
33	Room	Nitrogen	2
21	Room	Air	1
Undiluted semen			
29	35	Air	2
22	Room	Air	2
16	Room	Helium	2
		Nitrogen	2
15	90	Nitrogen	2
	35	Air	1

Table 2. Effect of length and temperature of storage of honey bee semen on success of queen insemination.

Days in storage	No. of queens that laid		No. of queens that died
	Fertile eggs	Infertile eggs	
Stored at room temperature			
68*	1	1	2
44	6	1	4
43	2	1	
31*	3		
29	1		3
Stored at 35°F			
43		2	5
31*		4	
29			5
Stored at 90°F			
68*		1	1
31*	1		2
27	3	4	2

* Chlortetracycline added.

discolored or more viscous than normal.

In the second experiment the effect of temperature on the fertilizing capacity of the sperm was studied by storing the sperm in sealed tubes at room temperature and 35° and 90°F from 27 to 68 days. In this experiment 55 queens were inseminated. Thirteen that had been inseminated with semen stored at room temperatures (72° to 86°F) and four inseminated with semen stored at 90°F produced fertile eggs (Table 2). No fertile eggs were obtained from queens inseminated with semen stored at 35°F. Storage was considered successful if at least half the eggs of each queen were fertilized. Sperms stored 4 weeks or longer at room temperatures or above remained viable. Microscopic examination showed that a number of dead queens had living sperms in the spermatheca. The use of better techniques for sterilization and an antibiotic, chlortetracycline, reduced contamination by microorganisms.

Four tubes each containing semen collected from about 20 drones were sent by regular mail to Madison, Wis. Of ten queens inseminated by O. Mackensen with this semen, one died, eight laid all fertile eggs, and one laid both infertile and fertile eggs. Counts of sperm cells in the spermathecae of the nine live queens compared favorably with counts in queens that had received an equivalent amount of fresh semen.

Why these sperms remained viable for such long periods is not known. An explanation of their longevity may be applicable to storage of sperm of other animals. Studies of the artificial preservation of sperm of domestic animals, especially the bull, have received considerable attention since the discovery that glycerol acts as a protective agent (*5*), permitting successful storage at low temperatures. Most of the work on mammalian sperm preservation has been done at very low temperatures, and successful inseminations have been made with sperm frozen 3 years (*6*). Sperms have remained alive for relatively long periods in the ligated epididymis of a number of mammals (*7*), and recently some success has been obtained in the preservation of bull semen at above-freezing temperatures when a continuous flow of physiological fluid passed dialyzing tubes containing the semen (*8*).

Mann (*9*) has shown the importance of fructose in the metabolism of semen. We have found fructose to be present in fresh bee semen, but it is rapidly metabolized, and 40 minutes after ejaculation it has disappeared. Obviously, a supply of fructose is not necessary to survival of honey bee sperm, and it is surprising that sperms can survive under so many different environmental conditions. It may be that sperms stored in the spermatheca receive little or no nourishment and have an extremely low metabolic rate. If so, it should be possible to develop a successful method for storing sperms for long periods of time.

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25 January 1960

Effects of Deprivation and Scheduling on Water Intake in the White Rat

Abstract. Two studies are presented to demonstrate that the consummatory behavior of drinking in the rat is under the control of duration of water deprivation and that intake after deprivation is related to variation in the scheduling of the animals' opportunities for drinking.

Miller (*1*) has recently pointed out that different measures of what is held to be a unitary process—for example, the drive state of thirst—on occasion show rather wide variation one from another. The present report (*2*) advances evidence that such variation occurs not only when different classes of behavior are observed as operational measures of the same drive, but also under circumstances where a single indicator varies with changes in laboratory routines.

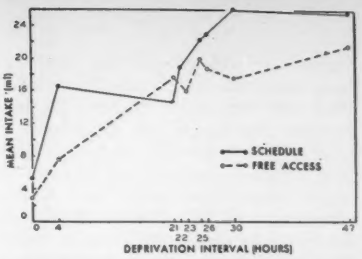

Fig. 1. Amount of water drunk during a test period of 1 hour following water deprivation of varying intervals for animals with free access to water and for animals with access to water limited to 1 hour a day.

In the first study, five albino rats of the Wistar strain were kept in individual cages in a darkened room over a period of several months. For the first 5 weeks of the study, the animals were permitted access to water for 1 hour a day (23-hour deprivation schedule); at the end of this period a series of deprivation tests was begun which extended over a period of 8 weeks (schedule condition). Two observations of drinking during a test period of 1 hour were made after deprivation of 4, 21, 22, 25, 26, and 30 hours, and one observation was made after 0 and one after 47 hours of deprivation. During this time, food was freely available to the animals, and, on days when a test was not scheduled, the animals drank at the usual 23-hour deprivation interval.

When these observations were completed, the animals were returned to free access to water as well as to food for 3 weeks, after which drinking tests were run at the same deprivation intervals used in the schedule condition. In this case, however, the animals were deprived of water *only* during the test interval (free access condition).

The results of the study are shown in Fig. 1. For animals on a free access base line, there is a relatively regular relation between deprivation interval and amount drunk. Intake in this condition shows a statistically significant increase between 0 and 4 hours of deprivation ($p < .05$), and between 4 hours of deprivation and all other intervals ($p < .01$), but there is no significant variation among deprivation intervals of 21, 23, 25, 26, and 30 hours. On the other hand, the amount drunk from a schedule base line shows a striking inflection near the deprivation interval associated with scheduled drinking. This inflection is sharp enough to produce statistically significant differ-

ences in intake between 21- and 22-hour deprivation, between 22- and 25-hour deprivation, and between 25- and 30-hour deprivation (all differences at $p < .01$). It is interesting, too, to note that, although a significant increase in intake occurs between 0- and 4-hour deprivation in the schedule condition, there is a nonsignificant *drop* in intake between 4- and 21-hour deprivation.

Scheduling of drinking on a once-a-day cycle seems to have the effect of reducing differences in intake between some relatively short period of deprivation (in this case, 4 hours) and longer periods up to the deprivation interval associated with scheduled drinking, and then of producing a sharp rise in intake at deprivation intervals longer than the one normally associated with scheduled drinking.

A second study was run in order to make a closer examination of intake at the lower end of the deprivation range. Ten albino Wistars were studied, five with free access to water at all times other than test intervals and five under 23-hour scheduled deprivation. Tests of water intake were run at 0, 1, 2½, 4, 5, 6, and 12 hours of water deprivation. In this study, the results of which are shown in Fig. 2, intake over the first 10 minutes of the test hour was analyzed because it had been shown that intake during the shorter period adequately represented intake over the entire hour.

It should be noted in Fig. 2 that, as the first study suggested, the rise of intake with deprivation appears to be sharper in the schedule group than in the free access group. For example, intake for the schedule group was significantly different ($p < .01$) between the lower deprivation intervals (1- and 2½-hour) and the higher intervals (6- and 12-hour). The rise of intake in the free access group, although regular, did not reach conventional levels of statistical significance.

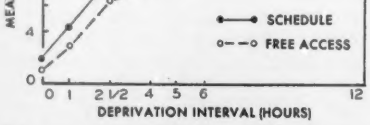

Fig. 2. Amount of water drunk during a test period of 10 minutes following water deprivation of varying intervals for animals with free access to water and for animals with access to water limited to 1 hour a day.

These studies confirm the observation of earlier workers (*3*) that a sharp rise in consummatory behavior takes place between 0 and 4 to 6 hours of deprivation, but they also suggest that part of this effect may be ascribable to the influence of caretaking schedules. Animals which have been "trained" to drink every 24 hours will show distortions of intake when compared with animals tested from a base line of free access to water. These distortions suggest caution in the use of hours of deprivation as a simple index of "thirst" and lend support to Miller's conclusions (*4*) about the complexity of drive measures in general.

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2 February 1960

Effects of Polyvalent Cobalt Salts on Human Cells in Tissue Culture

Abstract. Trivalent and hexavalent cobalt complexes have been tested to determine their ability to produce morphological changes in two different human cell cultures. Aortic intimal cells which produce mucopolysaccharides in vitro prove to be more susceptible than the nonmucopolysaccharide producing HeLa strain. This effect seems to be related to the reaction of polyanions such as ribonucleic acid, deoxyribonucleic acid, and chondroitin sulfate, with the complex cobalt salts.

The use of polyvalent cobalt salts for the precipitation of chondroitin sulfate and deoxyribonucleate (*1*, *2*) prompted us to investigate the effects of these salts on freshly grown cells capable of synthesizing sulfated mucopolysaccharides. Trivalent hexamminecobaltic chloride, $Co(HN_3)_6Cl_3$, and hexavalent hexol nitrate, $[Co(OH)_6(Coen_2)_3](NO_3)_6$, prepared by Maxwell Schubert (*3*), were examined for purity by x-ray powder diffraction patterns (*4*). The trivalent salt in dilute aqueous solutions forms precipitates with sulfated mucopolysaccharides and nucleic acids (RNA as well as DNA), and their respective protein complexes. At physiological *p*H and salt concentrations, only DNA is precipitated (*2*). The hexavalent salt will also precipitate hyaluronate and react with polyanions at physiological *p*H and ionic strength. That the cobalt salt may also form soluble complexes with polyanions is indicated by the dissolution of such precipitates by excess anion, and a specific increment at 235 mμ produced in a solution of hexamminecobaltic chloride by the presence of chondroitin sulfate (*3*).

To test whether these salts could precipitate the mucopolysaccharides or polysaccharide-protein complexes of aortic cells specifically, or both, several human aortas were extracted. Water soluble aortic mucopolysaccharide-protein complex was obtained by Schubert's method (*5*, *6*). This material was metachromatic, had a clear absorption peak at 280 mμ in a Beckman DU spectrophotometer, and contained 9.8 percent hexosamine and 8.2 percent nitrogen. It was obtained in a yield of 1 to 2 percent of dried, defatted aorta and sedimented as two peaks in the analytical ultracentrifuge. In aqueous solution the material gave precipitates with both polyvalent cobalt salts directly proportional to the amount of polysaccharide-protein complex added.

Due to the relative specificity of the reaction, we proposed to study whether these salts would (i) enter cells growing in tissue culture and (ii) manifest effects either in cytoplasmatic sites of RNA and mucopolysaccharide localization or in nuclear DNA itself. Two human cell types were used: (i) intimal cells from adult human aorta organ cultures (*7*) which produce mucopolysaccharides in short-term studies (*8*), and (ii) a commercial stock of strain HeLa (*9*) (Microbiological Associates, Inc.). Both cell types were maintained in 10 percent human serum and 90 percent Eagle's basal medium for periods of up to 120 hours.

The cobalt salts were dissolved in triple distilled water with a magnetic stirrer, sterilized by filtration through a UF fritted disc (Corning) in a Morton filter apparatus, and diluted to concentrations of 1 to 150 μg/ml in sterile Hanks' solution. After the drug was added, flying-coverslip type cultures were incubated at 36.5°C and observed periodically. The cultures were fixed in 10 percent Ringer's formalin and stained with May-Grünwald-Giemsa by W. Jacobson's method (Strangeways Laboratory), Alcian blue 8 GS-Ehrlich's hemalum, and periodic acid-Schiff.

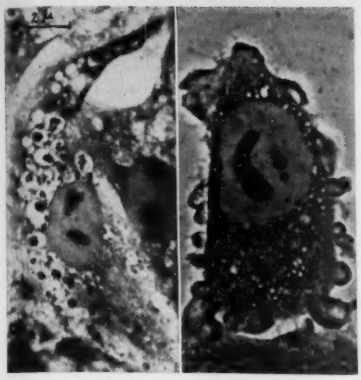

Fig. 1 (left). Cytoplasmatic vacuoles in human aortic intimal cells produced by trivalent hexamminecobaltic chloride. (Right) Intranucleolar granules in human aortic intimal cells produced by hexavalent hexol nitrate.

Half of the cultures were incubated for 12 hours after the drug was added, rinsed twice in Hanks' solution, and maintained in normal culture medium for the rest of the incubation time. As is shown in Table 1, different morphological nuclear and cytoplasmatic changes were observed in the aortic intimal cells and in HeLa cells at various concentrations of the salts. Trivalent hexamminecobaltic chloride at concentrations of 5 μg/ml produced in aortic intimal cells large cytoplasmatic vacuoles containing metachromatic granules after 12 hours of incubation, but no nuclear changes (Fig. 1, left). Such effects were not observed in the HeLa cells under similar conditions. Both cell types show growth inhibition and typical cytotoxic effects at concentrations of 100 μg/ml.

Table 1. Effect of various salts on human cell cultures.

Concn. (μg/ml)	Human aortic intimal cells		HeLa cells	
	Cytoplasmatic	Nuclear	Cytoplasmatic	Nuclear
	$Co(NH_3)_6Cl_3$			
1	±	−	−	−
5	+	−	−	−
25	++	−	−	−
50	+++	−	−	−
75	+++	−	−	−
100	+++	±	±	±
	$Co(OH)_6(Coen_2)(NO_3)_6$			
1	−	±	−	−
5	−	+	−	−
25	±	++	−	−
50	++	+++	−	±
75	+++	+++	±	+
100	+++	+++	+	++
	$CoCl_2$			
250	±	−	±	−
	$Ni_2(NH_3)_6Br_2$			
200	±	−	±	−

Hexavalent hexol nitrate (5 μg/ml) produced intranucleolar granules but no cytoplasmatic changes in human aortic intimal cells. At concentrations of 50 μg/ml these granules were very abundant and were accompanied by formation of large bubbles in the cell membrane but no vacuolization (Fig. 1, right). The nucleolar changes persisted for up to 120 hours of cultivation if these cells were exposed to the salt for 12 hours. The salt did not inhibit their mitotic index or prolong their generation time. In HeLa cells this salt produced nuclear picnosis, growth inhibition, and cytolysis, at concentrations of 100 μg/ml.

Control tests carried out with cobaltous chloride at similar concentrations showed growth inhibition with 250 μg/ml in both cell types without any of the morphological changes described above. Nickelous ammonium bromide produced similar results at concentrations of 200 μg/ml.

The effects of trivalent hexamminecobaltic chloride in aortic intimal cell cytoplasm can be interpreted as the induction of dilated vacuoles of endoplasmic reticulum with metachromatic granules. They are of interest because such findings have not been observed in HeLa cells, which in our experience have never shown production of mucopolysaccharides in tissue culture. No mitotic changes like those described for cobalt nitrate and other sulfhydryl reagents (*10*) were observed in any of these two cell lines. The effects of the hexavalent salt in the nucleolus and its persistence after removal of the salt from the culture medium are particularly pertinent considering the very low concentrations of this salt required to react in vitro. The action of these cobalt salts seems to be different from those observed by Levy *et al.* (*11*) with cobaltous sulfate in bacteria (*Proteus vulgaris*), which was able to arrest protein synthesis without halting RNA production.

Our results suggest that at very low concentrations trivalent cobaltous salts act upon cytoplasmatic RNA and sulfated mucopolysaccharide while the hexavalent salt reacts more specifically with nuclear RNA. These results support the hypothesis that complex cations capable of reacting with polyanions in solution exert morphological effects upon living cells at sites where polyanions are present, their degree and site of action depending on the cell type used and on the ability of the cells to produce mucopolysaccharides in vitro (*12*).

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8 February 1960

Hypothesis Concerning the Role of Follicular Contractions in Ovulation

Abstract. Autotransplants of ovarian tissue in the anterior chamber of the eye may be studied in the lightly anesthetized rabbit. Individual follicles in such implants, consisting of two to five follicles, have been observed to contract after subcutaneous injection of urine of pregnant women, after application of rat pituitary homogenate to the cornea, and 9 to 10 hours after cervical stimulation.

Although the presence of smooth muscle in the theca externa of the ovarian follicle of the rabbit was first reported by Thomson (*1*), neither he nor Guttmacher and Guttmacher (working with the sow) (*2*) succeeded in demonstrating functional activity in this muscle. The Guttmachers in 1921 attempted to induce the smooth muscle of the sow's ovarian theca externa to contract by means of electrical excitation and application of acid, alkali, and barium chloride solutions. These stimuli, where successful, were unphysiological, and although these workers suggested that the follicular muscle coat might be involved in the mechanism of ovulation, they failed to substantiate their hypothesis.

The observations reported here define a physiological role for the smooth muscle present in the theca externa. Such a role has been largely discounted because of two sets of experimental observations. Friedman (*3*) demonstrated that ovulation could occur in ovarian implants in the rectus muscle, and Hinsey and Markee (*4*) found that ovulation occurred in the denervated ovary.

Autotransplants of small numbers of ovarian follicles in the anterior chamber of the eye may be studied in the

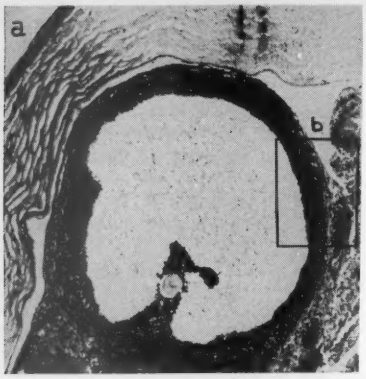

Fig. 1. Mature ovarian follicle in the anterior chamber of the rabbit eye. (*a*) Section through cornea; (*b*) section of wall shown in Fig. 2 (Gomori trichrome, about × 75).

lightly anesthetized rabbit. Implants were made into both eyes of several animals. Contractions were observed in one eye in each of two animals. The contractions involved individual follicles and caused changes in shape from the spherical to the elipsoidal form.

Implanted follicles grow, become filled with fluid, contain an ovum, and remain turgid for several months (Fig. 1). Figure 2 shows the organization of the follicular wall and the presence of spindle-shaped cells. We assume that these cells are the smooth-muscle cells that give rise to the contractions.

Contractions may be induced by several methods. Subcutaneous injection of 2 ml of ether-extracted urine obtained from pregnant women during their first trimester induced the appearance of contractions from 8 to 180 minutes after administration of the urine. The frequency of contraction was between 1 and 5 per minute. It is well established that ovulation in the rabbit occurs 9 to 10 hours after either mating or cervical stimulation (*5*). Nine hours after electrical stimulation of the cervix contractions appeared and persisted for approximately 2 hours. Marked contractions followed the application of a homogenate, prepared from an acetone-extracted rat pituitary, to the surface of the eye. This response is not obtained by direct application to the cornea of one drop of 1:100,000 epinephrine, one drop of 0.1-percent solution of acetylcholine, or one drop of 1 USP unit (oxytocic activity) of posterior pituitary extract. Hence it is very likely that the force responsible for the delivery of the ovum from the follicle is the series of contractions that pass over the ripe ovarian follicle, and that these are induced as a result of the release of luteinizing hormone.

A complete hypothesis of ovulation probably involves (i) release of the ovulation-inducing hormone, tentatively assumed to be luteinizing hormone (*6*); (ii) modification of the germinal epithelium by proteolysis (*7*); (iii) rapid swelling caused by increased secretory activity or depolymerization, or both, of the constituents of the liquor folliculi with a consequent increase in osmotic pressure (*8*); and (iv) contractions of smooth muscle in the theca externa to assist the rupture of the thecal wall at the stigma and facilitate the ejection of the intrafollicular contents. Such a mechanism appears necessary to account for the continued ejection of the intrafollicular contents after the pressures caused by osmotic forces are neutralized after rupture (*9*).

H. J. Lipner
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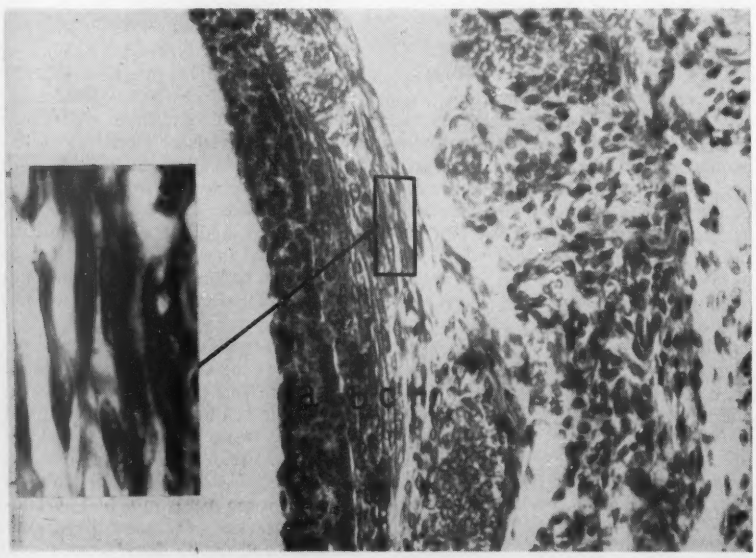

Fig. 2. Segment of thecal wall containing three layers: (*a*) granulosa; (*b*) theca interna; (*c*) theca externa (Gomori trichome, × 380). Inset shows *c*, at magnification of 1000, composed chiefly of spindle-shaped smooth-muscle fibers.

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9. This research was supported in part by FSU Research Council grant-in-aid No. 036-21 and Public Health Service grant No. A-1904.

23 December 1959

Finite Radiocarbon Dates of the Port Talbot Interstadial Deposits in Southern Ontario

Abstract. Three new finite radiocarbon dates suggest that (i) the thermal maximum of the Port Talbot interstadial occurred prior to 47,000 years before the present and (ii) the interstadial deposits were overridden by a glacial advance approximately 44,000 years before the present. To facilitate correlations with other areas, new rock-stratigraphic names are proposed for the Port Talbot type section.

New stratigraphic divisions of the last ice age, several of them older than the classical Wisconsin glacial stage, have been proposed by Dreimanis (*1*) since 1957. Unfortunately the radiocarbon dates of the principal new unit, the Port Talbot interstadial, were not finite. These dates (samples L-185A, L-217A, L-370A, L-440, W-100, S-7, and S-46; see *1*, *2*) ranged from older than 25,000 to older than 40,000 years. Therefore, several readers of the articles cited (*1*) and participants of the Friends of the Pleistocene 1959 field conference (*3*) have expressed doubt that this interstadial is younger than the last, or Sangamon, warm interglacial.

H. de Vries considered it worth while to try to obtain new radiocarbon dates, beyond the previous range of dating, at the Radiocarbon Laboratory of the University of Groningen. We collected gyttja from the Port Talbot interstadial site in the summer of 1958, but

subsequent examination showed that the sample was unsuitable because recent rootlets were present. We took a new sample, a monolith of about 25 pounds of gyttja, at the type section of the Port Talbot interstadial in July 1959 [6 ft inside the base of the cliff, at the 56-ft point of the geologic section, as shown in Fig. 2 of (*4*)]. The silt and gyttja surrounding the monolith were checked carefully, and no rootlets were found.

The first date, obtained from an unenriched sample of the gyttja (Gro-2570), was 47,000 ± 2500 years before the present (B.P.) (*5*). The final date (Gro-2597 and Gro-2601), after enrichment, was in very good agreement: 47,500 ± 250 years B.P.; the error of ± 250 is the statistical error in the activity only, and does not include the errors in the enrichment factors (*6*).

The new finite dates refer to the second half of the Port Talbot interstadial, for a palynological study (*7*) suggested that the gyttja was younger than the thermal maximum of this interstadial. The underlying silt, deposited during the first half of the interstadial, contained only a few organic remains, not enough for radiocarbon dating.

New fragments of wood were found in 1959, also in the till (f_1) which overlies the Port Talbot interstadial beds. Wood from this area has been previously dated (samples S-46 and L-440), also giving infinite dates. The new finite date (Gro-2580) is 44,200 ± 1500 B.P. (*8*). It is in very good agreement with the dates of the stratigraphically older gyttja, indicating the time when the Port Talbot interstadial deposits became overridden by a glacial advance in the central portion of Lake Erie basin.

The new Groningen radiocarbon dates make it possible to outline the preclassical Wisconsin stratigraphic units in southwestern Ontario more accurately than before. To facilitate correlation with other similar Pleistocene sections, Dreimanis proposes new local rock stratigraphic terms for the drifts overlying and underlying the Port Talbot interstadial beds, as shown in Table 1.

Frye and Willman (*9*) have recently proposed a revised classification of the Wisconsin stage of the Lake Michigan lobe, suggesting that the Farmdalian substage was a major interval of glacial withdrawal. Comparison of radiocarbon dates indicate that the Plum Point interstadial may be correlated with the Farmdalian. Only one glacial substage (the Altonian), older than the Farmdalian, has been proposed for the Lake Michigan lobe by Frye and Willman. The Port Talbot section suggests at least two glacial substages and one interstadial Wisconsin substage in the Lake Erie lobe area of southwestern Ontario before the Farmdalian (*10*).

Table 1. Wisconsin glacial and interstadial deposits at Port Talbot, Ontario.

New local stratigraphic names (with comments on C^{14} ages)	C^{14} Age and reference No.	Previous stratigraphic terms	
		1958 (*11*)	1957 (*12*)
Port Stanley drift		Upper tills (h) and (j), lacustrine clay (i)	Clayey upper till No. 4, lacustrine clay
Lake Erie interstadial beds (*13*)		Unconformity (lacustrine deposits elsewhere)	Lacustrine clay and silt
Catfish Creek drift		Sandy lower till (g)	Sandy lower till No. 3
Plum Point interstadial beds (not *in situ*–C^{14} dates of wood from the Catfish Creek drift)	24,600±1,600 (L-217B) 27,500±1,200 (W-177) 28,200±1,500 (L-185B)	Plum Point interstadial wood (reworked)	Plum Point interstadial
Southwold drift		Clayey gravel (f_2), till (f_1), lacustrine clay (e)	Gravelly lower till No. 2; gravel, lacustrine clay
Port Talbot interstadial beds		Port Talbot interstadial gyttja and silt (c)	Port Talbot interstadial gyttja and silt
a) end of the interstadial: wood in the Southwold drift	44,200±1500 (Gro-2580)		
b) second half of the interstadial: gyttja	47,500±250 (Gro-2597 and Gro-2601) 47,000±2500 (Gro-2570)		
Dunwich drift		Lowermost till (a) and varved clay (b)	Sandy lower till No. 1 and varved clay

Frye and Willman (*9*) have recently proposed a revised classification of the Wisconsin stage of the Lake Michigan lobe, suggesting that the Farmdalian substage was a major interval of glacial withdrawal. Comparison of radiocarbon dates indicate that the Plum Point interstadial may be correlated with the Farmdalian. Only one glacial substage (the Altonian), older than the Farmdalian, has been proposed for the Lake Michigan lobe by Frye and Willman. The Port Talbot section suggests at least two glacial substages and one interstadial Wisconsin substage in the Lake Erie lobe area of southwestern Ontario before the Farmdalian (*10*).

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ALEKSIS DREIMANIS
Department of Geology, University of Western Ontario, London, Canada

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6. ———, letter of 23 Nov. 1959. H. de Waard (12 Jan. 1960) mentions another reference number (Gro-2597/2601) with the same date.
7. A. Dreimanis, *Ohio J. Sci.* **58**, 74 (1958).
8. H. de Vries, letter of 24 Oct. 1959.
9. J. C. Frye and H. B. Willman, *Illinois State Geol. Survey Circ. No. 285* (1960).
10. This report grew out of correspondence between Hessel de Vries and me in 1959, and the first draft was on its way from Canada to the Netherlands when de Vries died. I assume responsibility for the wording of the report, hoping that opinions of the late Hessel de Vries have been expressed correctly. I appreciate very much the cooperation of H. de Waard, who is continuing de Vries' work at Groningen and who supplied the following additional information in a letter dated 8 March 1960:
A peat ball which I found on the Lake Erie shore at the Port Talbot interstadial type location has been dated 44,900 ± 1000 years B.P. (Gro-2619). This date is in good agreement with de Vries' dates for the Port Talbot interstadial deposits. De Vries suggested also redating of the Plum Point larchwood (see W-177 and L-185B in Table 1). The new date reported by de Waard is 27,250 ± 130 years B.P. (Gro-2625). It is in excellent agreement with the other two dates, and more accurate.—A. D.
11. A. Dreimanis, *Ohio J. Sci.* **58**, 82 (1958).
12. ———, *Science* **126**, 167 (1957).
13. These deposits are called the "Erie interstadial" in A. Dreimanis, *Contrib. Dept. Geol. Univ. Western Ontario No. 25* (1959), p. 28.

* Deceased.

1 February 1960

Letters

(Continued from page 1700)

improper, since many of the objective check-list items measured behavior more adequately designated as distress, disturbance, yearning, and displaced aggression. The term *disturbance index* could be substituted for *emotionality index* without semantic loss and, probably, without semantic gain.

HARRY F. HARLOW
Department of Psychology, University of Wisconsin, Madison

Small Colleges and Small Minds

The lack of interest of some teachers in research, discussed in the editorial of 8 January [*Science* **131**, 71 (1960)], is disturbing, but so also are some aspects of the editorial itself.

The heading "Small colleges and small minds" implies that these go together. This guilt-by-association technique is used several times in the editorial, though the man whose views are cited as the basis for the editorial is the president of a "small college." Is there evidence for a larger proportion of so-called small minds in small colleges?

The following statement is perhaps the worst part: "The core of the argument for scientific research . . . is that while there may be good research scientists who are not good teachers, the evidence is that there are no good teachers whose competence is not increased by good scholarship." Ostensibly this places the research scientist above reproach and leaves the incubus on the teacher. Actually, the statement is a *non sequitur*, and its converse is equally true—and unfair, in turn, to the research scientist. It is suggested that the reader substitute the word *editors* for *teachers* (he will find it equally correct). Competence in any profession would be increased by good scholarship, as the editorial in question seems to illustrate.

After this unfair statement the editor changes from "good scholarship" to "research" in the next sentence, which again helps to put the research scientist beyond criticism and implies criticism of the teacher. Had he chosen to make a straightforward statement of what he implies it might have read something like this: "While there may be good research scientists who are not good teachers, the evidence is that there are no good teachers who are not good research scientists." This is, I suggest, rather untenable.

In the next paragraph we are told: "A prominent figure on many campuses is the instructor who is forever marking exams, grading papers, and drawing curves representing his students' performance." With our present grading system instructors are inevitably marking exams, and so on, but the editor is depreciating the teacher with the guilt-by-association technique again, for he indicates that these instructors have "schemes" of a detrimental nature. But the scientist could be given the same unjust treatment, in very similar phrases: "A prominent figure in many research laboratories is the scientist who is forever looking at figures, evaluating data, and drawing curves representing his results. He is full of schemes . . . that if instituted would require the assistance of all his colleagues." Though, curiously, in this case involving one's colleagues becomes a virtue and is extolled under the name of "scientific teamwork."

But this is not all. Having implied that no research means no scholarship, the editor completes his degradation of the teacher in the next sentence by suggesting that those concerned with students' performance are even against reading books!

In the last paragraph the editor proposes the right question, but for the wrong schools, when he asks: "But why in small colleges should some instructors oppose the recognition of good research as a consideration second to good teaching?" In so far as this opposition exists, it is typical not of the small school but of the large school with an extensive graduate program, where some instructors want recognition of good research first and of good teaching second, if at all. In large measure the apparent hostility toward research in the small college is manifested by instructors who do not oppose research as such, but oppose the evaluation of good teaching as a consideration second to good research, because they have seen the unfortunate results of this practice, especially since all too often a department finds itself with "research scientists who are not good teachers."

WILLIAM K. NOYCE
University of Arkansas, Fayetteville

It was with considerable interest that we read your editorial "Small colleges and small minds," for here at Wilkes College the subject of research in small colleges has been of more than academic interest. Wilkes, I believe, would fulfill your criteria for a small independent liberal arts college. Our experience in the initiation and conduct of a research-teaching program has been satisfactory and rewarding. A summary of our findings may be of interest

Kodak reports on:

color lore that might have bored Goethe to death . . . what to do with wiggle-bearing paper . . . correspondence concerning dimerization of cyanamide

Gelatin responsibly dyed

A rich legacy of heuristic nonsense has been accumulating for generations in the next region of the electromagnetic spectrum over from the infrared, where the eye reigns supreme as the receptor and has qualified every seeing, thinking man to hold opinions. Even the mighty Johann Wolfgang Goethe, author not only of "Faust" but also of "Die Farbenlehre," put in his *zweipfennig* worth.

Amid rampant intellectuality, it has behooved us to tread lightly and confine our thinking to such *farbenlehre* as will fit us the better to flood the earth with color photography, myriad-colored Tenite plastics, color-locked Chromspun fibers, and Eastman textile dyes. Plus another field of dye art, tiny in economic comparison and disproportionately demanding in technical patience but important to those who, whatever their theories or purposes, wish to modify spectral distribution or overall intensity of light in systematic, quantitative, reproducible, simple, and inexpensive fashion. We refer to the celebrated little marvel of precision dye chemistry, the *Kodak Wratten Filter* of uniform gelatin, with or without glass mounting.

The reason we refer to it is that the new 20th edition of "Kodak Wratten Filters for Scientific and Technical Use," containing 81 pages of curves, data, and other useful information, is now obtainable from well-stocked photographic stores for 75¢ or from Eastman Kodak Company, Sales Service Division, Rochester 4, N. Y.

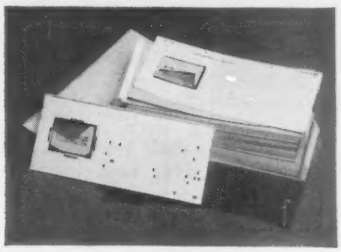
Back from the brink

When visitors come shuffling through your laboratory, it makes a terrific impression to show you are equipped for:

- Infrared absorption spectroscopy
- Ultraviolet absorption spectroscopy
- Visible absorption spectroscopy
- X-ray diffraction
- Nuclear magnetic resonance spectroscopy
- Fluorescence spectroscopy
- Raman spectroscopy
- Mass spectroscopy
- Gas chromatography
- Miscellaneous analytical methods like polarography
- Miscellaneous physical testing methods

Aside from creating an impression, these procedures generate strips of paper bearing wiggles. That there would be produced many such useful strips of wiggles to keep and compare had justified the acquisition of the instrumentation. The truer this has proved, the worse you may need help. We ourselves did. Fortunately, as we approached the brink of madness in coping with the sheer volume of spectrophotometric curves generated at the research laboratories of our division, Tennessee Eastman Company, we were able to call on our subsidiary, Recordak Corporation.

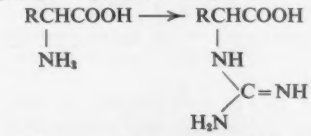
The Recordak people are experts at working out systems that combine micro-filming with punched cards. They say they would be willing to help you, too. They suggest you write them at 415 Madison Avenue, New York 17, N. Y., for a copy of "The Use of Aperture Cards for the Consolidation of Spectrophotometric Data."



Neat, like this.

Suggestion to Ames, Iowa

Cyanamide (note the "e"; very, *very*, VERY important) is not stable. On that, Walter R. Hearn of Iowa State University and we agree. Dr. Hearn is interested in guanidation of amino groups in peptides and protein, i.e.

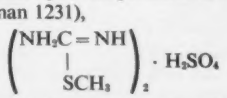


This can be accomplished with cyanamide ($H_2NC{\equiv}N$) and some of its derivatives. There was a problem. In thinking of well-known chemical houses with whom to take up a problem involving cyanamide, one doesn't necessarily think first of us, but Dr. Hearn had somehow formed the impression that we were friendly fellows. Another factor which might have contributed to his decision to write us was the fact that six bottles of cyanamide in his stockroom, which showed melting points as much as 150° higher than they were supposed to, happened to bear our P1995 label.

Well, sir, we *did* prove friendly.

We pointed in a friendly way to the "Practical" on that label as an open admission that the *Cyanamide* probably wasn't all cyanamide, though it had been originally. We said that to retard dimerization we kept our stock of *Cyanamide* under refrigeration and advised him to do likewise. We suggested he reclaim the undimerized portion of his stock by dissolving in ten parts or more of ether, filtering off any dimer, and concentrating the filtrate below 35°C at all times. We warned him not to dissolve in less ether because he'd get dimer into solution.

We also answered his question of why our *Cyanamide* (*Practical*) was 25 times as expensive as one of the cyanamide derivatives that he used, our *S-Methyl-2-thiopseudourea Sulfate* (Eastman 1231),



by explaining we did not make the latter from pesky cyanamide.

We further suggested how he might obtain the oxygen analog of this isothiouronium salt which he preferred for some of his guanidation operations. We referred him to a paper by one of our cousins at Kodak Limited in England (*Journal of the Chemical Society, 1955*, 3551), where cyanamide is by-passed by smooth methylation of urea with methyl toluene-p-sulfonate to give a good yield of readily isolated product. Finally we proposed that if he did not want to try this himself we would, for 96 bucks, cash on the barrelhead, deliver to him 500 grams of O-methyl-2-pseudourea sulfate.

That ought to teach Walter R. Hearn of Ames, Iowa, not to begin a letter with, "Since you have not acknowledged my letter of September 22, I thought perhaps you had gone out of business."

We thought everybody knew that we're still in business. After all, don't we keep advertising that there are some 3800 organic chemicals available from Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company)?

Prices quoted are subject to change without notice.

This is another advertisement where Eastman Kodak Company probes at random for mutual interests and occasionally a little revenue from those whose work has something to do with science

Kodak

TRADE MARK

to a general scientific audience. We published a fuller description in the *AIBS Bulletin* [**8**, 16 (1958)].

We found at Wilkes that the minimum prerequisites for establishing a research program are: teachers with an interest in conducting investigations and with the ability to communicate that interest to students; a sympathetic attitude on the part of the college administration; realization that the merit of a given piece of research is not measured only by the magnitude of the study or by the amount of technical apparatus it requires; selection of a problem suitable for investigation by a group; adaptation of student laboratory equipment for special needs and utilization of existing classroom space by appropriate planning; enlistment of the librarians of the college in seeking the cooperation of local hospital and industrial libraries and that of university and governmental library loan and microfilming services; use of undergraduate assistants—under supervision they have often proved as valuable as the average technician working solely for a salary; and, finally, invitation of scientists residing in the community to participate in the research-teaching program.

SHELDON G. COHEN
CHARLES B. REIF
Department of Biology, Wilkes College, Wilkes-Barre, Pennsylvania

From my limited experience it would seem that problems of opposition to research on the part of any college staff member are minor. The real problem, as usual, is one of finance or stimulation of interest. Small colleges are not even in the running when it comes to the money spent by the larger institutions just to line up federal grants, to lobby the legislature, or to secure research money from industry or philanthropical organizations.

The faculty and students in most colleges are usually a step or two ahead of the administration and sources of income both as to the desirability of research and the time and minor facilities necessary to be devoted to studies.

A. D. MOINAT
Colorado State College, Greeley

It is easy to agree with Wiggers [*Science* **131**, 942 (25 March 1960)] that "larger colleges do not have a monopoly on students with ability, curiosity, and desire." This is a truism. What he overlooks in asserting that smaller colleges do not supply their share of the scientific talent of the country is that they have, in fact, provided a disproportionate share of scientific personnel. As reported in the October 1948 issue of *Fortune* and in the study of the origins of American scientists by R. H. Knapp (1952), one of the anomalous conclusions was that most Ph.D.'s in science received their undergraduate training in small and even obscure colleges. The productivity of these smaller colleges, measured as a proportion of the number of graduates, contrasts strangely with the low productivity of larger institutions famed for research and staffed by prominent scientists. Most of the faculty members responsible for stimulating these undergraduates to pursue scientific careers were not themselves well known as researchers.

Reportedly, in recent years the larger and wealthier institutions have performed somewhat better in fulfilling their obligation to provide inspiration, challenges, and opportunities leading to scientific careers.

It may be that the commonly overworked science teacher in the smaller college somehow transferred his own thwarted research ambitions and motivation to students who showed promise and interest in a scientific career. Whatever the explanation, it is unfair to castigate these persons and institutions for not producing scientists when they have produced scientists and scholars in larger proportion than their numbers, faculty, facilities, or financial status would seem to warrant.

ROBERT P. MCINTOSH
University of Notre Dame, Notre Dame, Indiana

The responses of Wiggers and of Allen to the editorial "Small colleges and small minds" reflect two views on the subject of teaching and research in the small college neither of which are entirely realistic. I am in a position to know that research of a serious nature not only can be pursued in a small college but serves as a potent stimulus to student curiosity and interest and gives the staff member a sense of fulfillment which teaching alone seldom does. Allen's rather cynical comment that "teaching should be more than a meal ticket for researchers" should not be considered a universal attitude among college scientists.

At this college and in this department the research program during the academic year is necessarily curtailed because of teaching duties. But there are virtually 4 months of summer during which research is pursued without interruption. In the early stages support must be had from the college itself, but if the caliber of the research is sufficiently high, outside support in the form of grants is available.

No matter how much this subject of college research is kicked around,

however, the fact remains that, wherever he may find himself, the scientist who pursues research out of a deep inner urge to do so will find ways and means of satisfying his needs. It is this individual and not the opportunist who does research for what he may get out of it in the way of promotion who will, with some support, reveal the now almost hidden sources of research talent that are present in our small colleges.

J. KENNETH DONAHUE

Department of Biology, Utica College of Syracuse University, Utica, New York

Obesity and Steroid Hormones

In the 5 June 1959 issue of *Science* [**129**, 1546 (1959)] there appeared a report on "Storage of steroid hormones by adipose tissue in two experimental obesities," by Zomzely, Asti, and Mayer.

This otherwise admirable article contains an inaccuracy in its reference to the studies of Gallagher, Fukushima, Barry, and Dobriner [*Recent Progr. in Hormone Research* **6**, 131 (1951)]. In the last three sentences of the article by Zomzely *et al.* it is stated that a large amount of fat in obese individuals may favor retention of their own steroid hormones, and that storage of administered hormones in fat depots might have therapeutic significance. The work of Gallagher *et al.* is cited in connection with this statement.

I did not detect any reference to this point of view in my examination of the article by Gallagher *et al.* However, an explicit prediction of the findings contained in the *Science* article, and of the probable importance of this in the control of obese patients, was adumbrated by S. G. Margolin in a communication to me in 1953.

With Margolin's permission, his hypothesis was summarized in a chapter of a book in the editing of which I collaborated at about that time [E. D. Wittkower and R. A. Cleghorn, Eds., *Recent Developments in Psychosomatic Medicine* (Lippincott, Philadelphia, 1954)]. It may be of some importance to draw attention to this, particularly because of one suggestion contained therein—namely, that in the dieting of obese patients, a reinforcement of the biological instinctual appetite to eat may occur with the release of steroid hormones as the patient reduces weight. No attention seems to have been paid to this point in the literature, and it is a suggestion of sufficient interest to be entertained.

ROBERT A. CLEGHORN

Allan Memorial Institute of Psychiatry, Montreal, Canada

Meetings

Pacific Division, AAAS

The 41st annual meeting of the Pacific Division of the AAAS will be held at the University of Oregon, Eugene, 13–18 June 1960. Arrangements are in charge of a local committee headed by Bradley T. Scheer, head of the department of biology, to whom inquiries regarding the meeting should be addressed.

The president of the Pacific Division is Henry P. Hansen, dean of the Graduate School, Oregon State College, Corvallis. The retiring president is Henry Eyring, dean of the Graduate School of the University of Utah, Salt Lake City. The president elect is Wilbert A. Clemens, professor of zoology, emeritus, University of British Columbia, Vancouver.

Among highlights of the Eugene meeting will be the divisional symposium on man's exploration of space and the presidential address, entitled "Cycles and Geochronology."

The 40th annual meeting, held in San Diego, 15–19 June 1959, was hosted by five local institutions: San Diego State College, the Scripps Institution of Oceanography (University of California), the U.S. Navy Electronics Laboratory, the Zoological Society of San Diego, and the San Diego Society of Natural History. Local arrangements were ably handled by a committee headed by George E. Lindsay, director of the San Diego Museum of Natural History. Most of the sessions were held on the campus of San Diego State College.

Outstanding among the many excellent programs were the divisional symposium on results of the International Geophysical Year, moderated by Joseph Kaplan, chairman of the U.S. Committee for the IGY, and the presidential address of Henry Eyring entitled, "The Chemist Looks into the Future."

The total registered attendance of 1448 included representatives of 37 societies affiliated with the Pacific Division, a number of societies not affiliated, and 207 persons who failed to note their society affiliations. The registrants represented 17 states, the District of Columbia, the Virgin Islands, the Canal Zone, and 13 countries other than the United States and Canada.

ROBERT C. MILLER
California Academy of Sciences, San Francisco, California

Forthcoming Events

July

3–5. American Assoc. of Colleges of Pharmacy, Boulder, Colo. (G. L. Webster, College of Pharmacy, Univ. of Illinois, Chicago 12)

4–8. Polarization Phenomena of Nucleons, symp., Basle, Switzerland. (K. P. Meyer, Physikalisches Institut der Universität Basle, Klingelbergstr. 82, Basle)

5–9. Goiter Conf., 4th intern., London, England. (J. C. McClintock, 149½ Washington Ave., Albany 10, N.Y.)

6–15. Entomological Conf., 7th Commonwealth, London, England. (Commonwealth Inst. of Entomology, 56 Queen's Gate, London, S.W.7)

10–14. Pan American Tuberculosis Cong., 12th, Bahia, Brazil. (F. D. Gómez, 26, de Marzo, 1065, Montevideo, Uruguay)

11–12. Response of Materials to High Velocity Deformation, conf., Estes Park, Colo. (AIME, 29 W. 39 St., New York 18)

11–15. British Dental Assoc., annual, Edinburgh, Scotland. (Secretary, British Dental Assoc., 13 Hill St., Berkeley Sq., London, W.1, England)

11–15. Royal Medico-Physiological Assoc., annual, London, England. (A. B. Monro, 11 Chandos St., Cavendish Sq., London, W.1)

11–16. Inter-American Nuclear Energy Commission, 2nd meeting, Petropolis, Rio de Janeiro, Brazil. (J. D. Perkinson, Jr., Inter-American Nuclear Energy Commission, c/o Pan American Union, Washington 6)

11–18. Earthquake Engineering, 2nd world conf., Tokyo and Kyoto, Japan. (K. Muto, Organizing Committee, 2nd World Conf. on Earthquake Engineering, Science Council of Japan, Ueno Park, Taito-ku, Tokyo)

11–22. Grassland Cong., 8th intern., Reading, Berks, England. (British Grassland Soc., Grassland Research Inst., Hurley, North Maidenhead, Berks)

15–22. Mycology, 6th Commonwealth conf., London, England. (Commonwealth Mycological Inst., Ferry Lane, Kew, Surrey, England)

18–22. International Conf. on Congenital Malformations, London, England. (S. E. Henwood, Intern. Medical Congress, Ltd., 120 Broadway, New York 5)

18–22. Peaceful Application of Nuclear Energy, 3rd Inter-American symp., Petropolis, Rio de Janeiro, Brazil. (J. D. Perkinson, Jr., Inter-American Nuclear Energy Commission, c/o Pan American Union, Washington 6)

18–23. Endocrinology, 1st intern. cong., Copenhagen, Denmark. (G. Pincus, 1st Intern. Cong. of Endocrinology, Worcester Foundation, Shrewsbury, Mass.)

18–25. French Assoc. for the Advancement of Science, 79th cong., Grenoble. (Association Française pour l'Avancement des Sciences, 28 rue Serpente, Paris 6°)

19–22. International Conf. on Scientific Problems of Crop Protection, Budapest, Hungary. (Z. Király, Research Inst. for Plant Protection, Budapest)

21–27. Medical Electronics, 3rd intern. conf., Olympia, London, England. (Secretary, Institution of Electrical Engineers, Savoy Pl., London, W.C.2)

23–28. Otolaryngology, 7th intern. cong., Paris, France. (H. Guillon, 6, avenue MacMahon, Paris, 17°)

24–19. Modern Physical Theories and Associated Mathematical Developments, Boulder, Colo. (K. O. Friedricks, New York Univ., 25 Waverly Pl., New York)

25–6. International Assoc. of Physical Oceanography, 13th general assembly, Helsinki, Finland. (B. Kullenberg, c/o Oceanografiska Institutet, P.O. Box 1038, Goteborg 4, Sweden)

26–28. Poliomyelitis, 5th intern, conf., Copenhagen, Denmark. (S. E. Henwood, International Poliomyelitis Congress, 120 Broadway, New York 5)

27–12. Mathematical Statistics and Probability, symp., Berkeley, Calif. (A. P. Burroughs, Air Force Office of Scientific Research, Research Information Office, AFOSR/USAF, Washington 25)

28–29. Computers and Data Processing, 7th annual symp., Estes Park, Colo. (W. H. Eichelberger, Denver Research Inst. Univ. of Denver, Denver 10, Colo.)

30–6. Institute on Religion in an Age of Science, 7th annual conf., Star Island, N.H. (R. Burhoe, American Acad. of Arts and Sciences, 280 Newton St., Brookline 46, Mass.)

31–5. Alcohol and Alcoholism, 26th intern. cong., Stockholm, Sweden. (A. Tongue, Bureau International contre l'Alcoolisme, Case Gare 49, Lausanne, Switzerland)

(See issue of 20 May for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.*

■ SHAFT-ANGLE ENCODER gives angular-position data in 17-digit cyclic-binary code with accuracy said to be ±1 digit. The encoder consists of a glass disk coded by opaque and transparent segments. A flash lamp illuminates a radius of the disk for photoelectric reading. The 10-in. diameter unit includes 17 transistorized amplifier channels and power supplies. Parallel output is standard but serial output can be provided. (Wayne-George Corp., Dept. Sci576, 588 Commonwealth Ave., Boston 10.)

■ RADIOLOGICAL TEST SUBJECTS are plastic man-equivalent stand-ins that duplicate body reactions. An emitter model and an absorber model are both equivalent to an average man in size and contour. They are made of radiolucent shells and filled with a solution having the radiation-interaction properties of human soft tissues. The absorption model is equipped with a skeleton and a system of parts and ducts for insertion of dosimeters within the long bones and spinal column and into most soft-tissue regions. The emitter or calibration model does not contain a skeleton but is fitted with any specified combination of organs. Each organ may be loaded separately with radioactive materials, and the body as a whole may be given a separate generalized burden. (Atomic Accessories Inc., Dept. Sci578, 244-02 Jamaica Ave., Bellerose 26, N.Y.)

■ GAS-SAMPLING VALVES for chromatography applications are six-way types designed for injection of precise volumes. Holdup volumes are said to be less than 0.1 ml. Five models available include one heated type that allows injection of compounds of boiling points to 250°C. (Wilkins Instrument & Research Inc., Dept. Sci584, P.O. Box 313, Walnut Creek, Calif.)

■ WATTMETER combines a Hall-effect device with a contact-making D'Arsonval movement. The Hall-effect solid-state device furnishes a voltage output proportional to the power in a load circuit. This voltage is fed into a meter-relay movement calibrated directly for power measurement. Full-scale sensitivity of 500 mw is available in standard instruments with 100-mw sensitivity said to be possible. Both d-c models and a-c models for frequencies up to 1000 cy/sec can be supplied. (Assembly Products, Inc., Dept. Sci585, Chesterland, Ohio.)

■ TEMPERATURE CONTROL CHAMBER is designed to make possible tensile and compressional tests at a temperature range from −95° to +1000°F. When used with the manufacturer's tensile testing instruments, the chamber permits the use of most normal jaws and accommodates 10-in. sample with up to 80 percent rupture extension. Temperature constancy is said to range from ±1° to ±2°F over the entire range. Working space dimensions are 12 by 9.5 by 26 in. (Custom Scientific Instruments Inc., Dept. Sci588, Kearny, N.J.)

■ BERYLLIUM OXIDE CERAMIC TUBING is available in lengths to 21 in. from stock and to 24 in. on special order. Standard inside diameters are 5 mm, and ¼, 3/8, and 7/16 in. Tubes are supplied with both ends or only one end open. The tubing is said to be essentially gas tight, and melting point is given as 4650°F. (National Beryllia Corp., Dept. Sci582, 4501 Dell Ave., North Bergen, N.J.)

■ CODE CONVERTER is a solid-state device that will translate up to 14 bits in Gray code to ordinary binary code, simultaneously providing the binary complement as well. The encoder output is filtered and clipped to eliminate brush-bounce noise. Dimensions are 4¼ by 6¾ by 6¼ in. (Datex Corporation, Dept. Sci583, 1307 S. Myrtle Ave., Monrovia, Calif.)

■ VACUUM CALIBRATION TEST BENCH consists of a 2-in. high-vacuum pumping system with air-drying column, pressure-regulating needle valve, liquid-nitrogen traps, a vertical manifold with individually valved parts, a three-range McLeod gage with CO_2-driven mercury system, and a precision manometer. The manometer covers pressures from atmospheric to 5 mm Hg. The McLeod gage reads pressures from 17 mm Hg to 0.3 μ. Accuracy of the latter is said to be ±1 percent of reading. (NRC Equipment Corp., Dept. Sci587, 160 Charlemont St., Newton 61, Mass.)

■ INORGANIC INSULATION PAPERS are prepared from synthetic mica. Three forms are available. One is a 100-percent synthetic mica paper; the other two contain fibers. Thickness presently available is 5 mils. Density may vary from 0.6 to 1.7 gm/cm^3. Melting point is 1900°F if heated in a closed system, but the material converts to a higher-melting ceramic body when heated in an open system. Infrared transmission is less than 3 percent from 0 to 15 μ for a 7-mil sheet. Thermal conductivity may be 0.3 to 1.5 Btu in./ft^2 hr °F. (Minnesota Mining and Manufacturing Co., Dept. Sci580, St. Paul 6, Minn.)

JOSHUA STERN
National Bureau of Standards, Washington, D.C.

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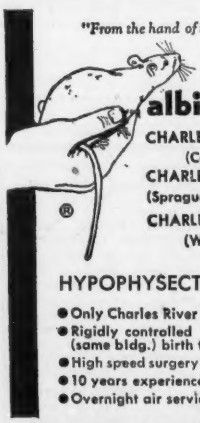

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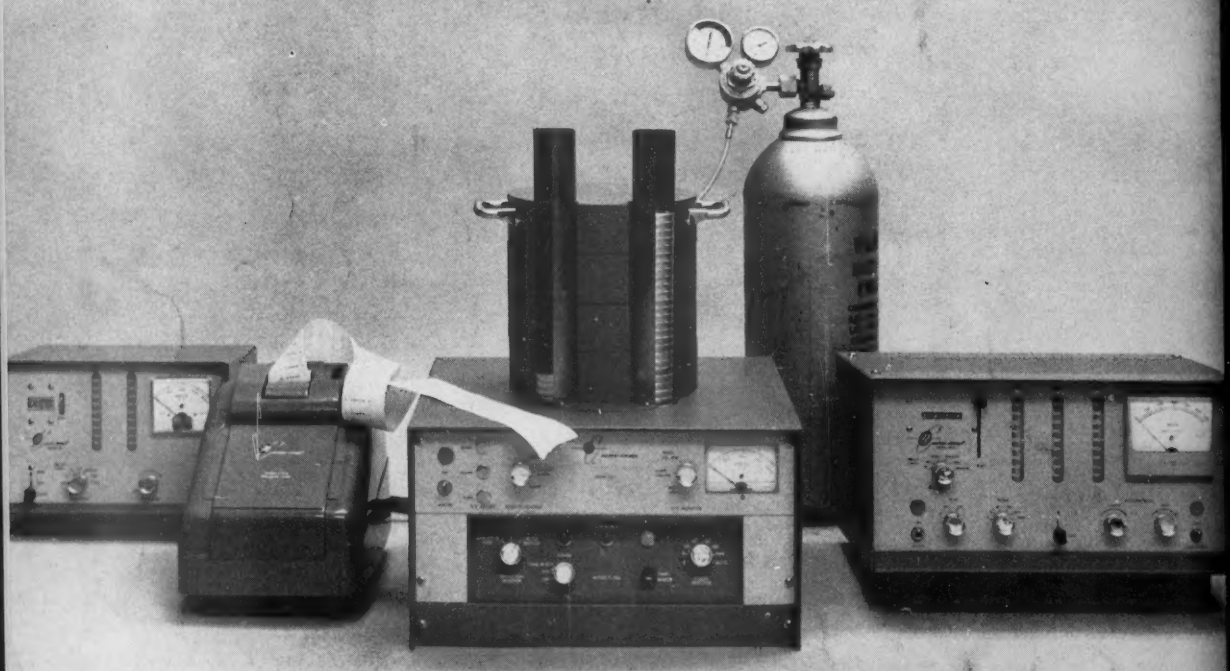

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